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Michael Brian Schiffer

The Archaeology of Science

Studying the Creation of Useful Knowledge

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Author's Sculpture, *Time Dilation*, Inspired by Einstein's Special Theory of Relativity (author photograph)

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Preface

For many years I had contemplated writing a book about science, having spent much time immersed in the science and technologies of early electricity. I had also done desultory reading in the history and philosophy of science and even wrote during the summer of 2002 a manuscript of poor quality. But not until October of 2011 did inspiration strike, allowing me to revisit and reconceive the book project. To pass the time during an all-day train trip from northern Virginia to Boston, I read Harrison and Schofield's (2010) *After Modernity: Archaeological Approaches to the Contemporary Past*. From this engrossing work I learned that a few archaeologists had been documenting the remains of twentieth-century scientific activities, including the testing of nuclear weapons in the Nevada desert.

Discovering that other archaeologists had already begun the study of modern science was the impetus I needed to envision *The Archaeology of Science*, which would showcase traditional strategies as well as new case studies. By the time the train reached Boston, I had written on a yellow pad many pages of notes and a provisional outline. This book would explore the diverse research activities that archaeologists use to study science—ancient and modern. Upon returning to Tucson in January, I began work on this project in earnest; the research was exciting and the writing most pleasurable.

Behavioralists have long maintained that archaeologists investigate the science embodied in traditional technologies (e.g., McGuire and Schiffer 1983; Schiffer and Skibo 1987, 1997). Toward this end we employ—singly and in combination—experiments, ethnoarchaeology, and archaeometry to tease out the generalizations that people had discovered about, for example, the properties of materials and processes of artifact manufacture and use. However, as Harrison and Schofield show, our efforts need not be confined to studying the science of ancient technologies and of traditional peoples. Indeed, my earlier forays into electrical science and technology demonstrated that the science of early modern and modern technologies implicates a host of new research questions whose answers may be sought in diverse lines of historical and archaeological evidence. The archaeology of science, then,

embraces the old and new, the exotic and familiar—in short, scientific activities of all times and all places.

To give some coherence to this expansive vision, I have rethought from a behavioral perspective the general concepts of scientific knowledge and reconsidered the relationships between science and technology. My notions on these topics, presented in chapters "Science: A Behavioral Perspective" and "Varieties of Scientific Knowledge," promote a holistic view of science and of the archaeology of science. The remaining chapters explore what the archaeology of science has been (Part II) and what it is becoming (Part III).

Instead of trying to survey all previous work, a patent impossibility that would result only in strings of "drive-by" citations, I present extended examples and lengthy case studies that, in light of the discussions in chapters "Science: A Behavioral Perspective" and "Varieties of Scientific Knowledge," illustrate archaeological strategies for researching science. I hope that the case studies will interest the reader as much as they have the writer, for they handle intriguing subjects such as the first machine that generated electricity, the Polynesian colonization of New Zealand, and a nuclear-thermal engine for rockets.

Throughout the chapters, especially in Part III, I offer suggestions about research potential, often in the form of substantive questions, which perhaps the next generation of archaeologists will design projects to answer.

This book's main target audience is advanced undergraduates and graduate students. Indeed, *The Archaeology of Science* could serve well as a text in a seminar-style course whose discussions focus on the several stages of student-initiated projects undertaken in the spirit of this book.

Although this book is aimed at other archaeologists and their students, I believe that the provocative arguments in Part I, especially, could potentially interest scholars in every discipline that studies science: history, philosophy, sociology, and cultural anthropology. Among the many features that may give the work some traction in other disciplines are the following.

1. A fully general definition of science is provided that applies to all human societies (see chapter "Science: A Behavioral Perspective").
2. I bring into the scope of science studies the activities of ordinary people, apparatus consisting of mundane objects, and the kinds of scientific knowledge that render predictable people's interactions with the material world (see chapter "Science: A Behavioral Perspective").
3. New insights are offered into the relationship of science and technology by focusing on projects (see chapter "Science: A Behavioral Perspective").
4. I treat at length the several kinds of scientific knowledge that enable predictions, including recipes (see chapter "Varieties of Scientific Knowledge").
5. A bridge is built between the "unobservables" invoked by traditional (folk) theories and the theories of modern science by means of the concept of "quasi-natural entity" (see chapter "Varieties of Scientific Knowledge").

Finally, in explaining this project to friends and family, I realized that this book may have an audience beyond the academy because the case studies in Parts II and III are so fascinating and eminently accessible to the general reader.

Alexandria, VA, USA Michael Brian Schiffer

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Acknowledgments

During the fall of 2001, I was a Senior Fellow in the Dibner Institute at MIT where I was able to interact with more than two dozen junior and senior fellows who represented a variety of disciplines studying science and technology. On the basis of lunchtime presentations and discussions it became clear to me that an archaeological perspective might make useful contributions. However, the draft I wrote during the following summer was, as Jennifer L. Croissant kindly pointed out, a polemic that would inflame rather than inform. She also supplied constructive suggestions that have helped me in crafting the present work. I am greatly indebted to Jen for sparing me the embarrassment of trying to get that horrid manuscript published.

In January of 2012, I put out a call to historical archaeologists, asking for information about projects that had studied scientific activities. I received dozens of replies containing many citations, pfds, and other information. From these I selected some examples and case studies that appear in Parts II and III. A mere thank you cannot express my delight in the collegiality of historical archaeologists, including Lyle Browning, Henry Cary, Craig Cessford, Sarah E. Cowie, Melissa Diamanti, C. J. Evans, Denis Gojak, Christina J. Hodge, Silas Hurry, Nicholas M. Lucchetti, Natascha Mehler, Robin O. Mills, Michael R. Polk, James Symonds, and Steven Walton.

In writing *The Archaeology of Science* I have been fortunate to obtain from various colleagues insightful critiques of several chapters. I thank them one and all for giving generously of their time and expertise in support of this project (see individual chapters for specific acknowledgments). I single out for special thanks Deborah J. Warner who provided instructive comments on the entire manuscript, helping me to express ideas with greater clarity and brevity.

I thank Arthur P. Molella, director of the Lemelson Center, National Museum of American History, Smithsonian Institution, for giving me a home away from home as a Research Associate. During our Tuesday lunches at the Smithsonian, Deborah J. Warner and Peggy Kidwell supplied very helpful feedback on ideas I was developing for this book. Discussions with Lars Fogelin helped me to refine my definition of science.

As always, Annette, my wife and best friend, gives me love and encouragement, provides feedback on ideas, endures graciously my random bouts of exuberance and despair, and makes our lives a magical journey.

Alexandria, VA
December 2012

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Part I

Introduction

Renowned for theories and equations that helped make modern physics, Newton and Einstein are icons of Western science, their Herculean achievements celebrated in the academy and in popular culture. Regrettably, the high visibility and veneration of stars like Newton and Einstein foster several misunderstandings about the nature of science.

First is the impression that science-making is an activity fit only for geniuses. As Kuhn (1970) reminds us, most scientific activity is actually routine problem-solving, presumably done by people less gifted than Newton and Einstein. Second is the belief that the products of scientific activity must be expressed mathematically. In fact, much scientific knowledge is neither quantitative nor quantifiable. Moreover, significant *qualitative* knowledge is present in natural history, much biology and geology, most science in ancient states, and all science in traditional societies. Third is the notion that the major goal of scientific activity is to create theories. Theories are of course important, even in traditional societies, but scientific activity generates knowledge of many kinds. In countering these and other misunderstandings, this book presents a general conception of science, *applicable to all societies*, that includes the contributions of ordinary people, recognizes the importance of qualitative findings, and handles many varieties of scientific knowledge.

Let us begin by considering science as both a process and a product. As a *process*, science consists of varied activities, or practices, for fashioning many kinds of useful—strictly speaking, *potentially* useful—knowledge.[1] In fact, science is not *a* process but many processes. There is no single scientific method because people create new knowledge in many ways for many uses in diverse societal contexts. A person may begin with an observation, puzzle, anomaly, problem, hunch, question, theory, dream, model, analogy, or new artifact, and can reach an outcome inductively,

[1] I considered using terms like "natural knowledge," which dates back to the late seventeenth century, and "practical knowledge," of more recent vintage, to denote the products of science. I rejected the former because it ignores the agency of humans in knowledge-creation activities and the latter because the term "practical" is highly problematic (Schiffer 2008, chapter 1).

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deductively, abductively, and even by nonlogical means. The processes of science-making are of archaeological interest because virtually all involve material phenomena, usually artifacts (cf. Hankins and Silverman 1995; Rothbart 2007).

The expected *product* of scientific activities is new knowledge: descriptions and generalizations—qualitative and quantitative—about the material world that permit predictions. This predictive capability makes scientific knowledge useful by enabling the forward motion of activities (cf. Atran and Medin 2008; Reichenbach 1966; Schiffer and Miller 1999). Thus, no human society has failed to create scientific knowledge, for it empowers people to conduct activities on the basis of their predicted outcomes. Through scientific knowledge, whether possessed implicitly or explicitly, people collect plants that can be eaten and select stones that can be chipped. Likewise, scientific knowledge allows engineers to design bridges that will survive strong winds and heavy loads and physicists to predict that ramping up the power in CERN's Large Hadron Collider will not generate a swarm of black holes and destroy Earth.

Scientific knowledge makes possible, though it does not guarantee in every instance, effective human interactions with artifacts and with living and nonliving phenomena of the natural environment. And it matters not whether ancient foragers were making digging sticks or hundreds of corporations are making a nuclear aircraft carrier because activities of every kind and complexity embody scientific knowledge. By facilitating activities—and thus activity change—the products of science are of archaeological interest.

With its high-tech research tools, complex organizational structures, and many specialized social roles, modern science merely elaborates processes established during the Paleolithic for creating useful knowledge.[2] In every society, ancient or modern, people playing different social roles create and use different kinds of scientific knowledge—as "situated knowledge" (Wylie 2003:31) or "socially distributed" knowledge (e.g., D'Andrade 1995:208; Hutchins 1995). In traditional societies, hunters employ their own activity-specific descriptions, generalizations, and artifacts—as do gatherers. In industrial societies, the differentiation of scientific knowledge is extreme because the conduct of virtually every activity requires specialized knowledge. It follows that societies having highly diverse activities also have highly differentiated science.

The Study of Science

The specialists in industrial societies include scholars in many disciplines who study the history, processes, and products of science. Indeed, science is probably studied in more disciplines than any other academic subject and is also a major focus of

[2] People obviously create many other kinds of knowledge having their own domains of use, from social science to theology, but they are not treated in this book.

interdisciplinary programs that go by the acronym STS (science-technology-society). The following list includes perspectives commonly found in science studies.

1. History of an idea or generalization such as the origins of field theory (Williams 1980).
2. A survey of science during a specific period in a particular country or region; e.g., the "scientific revolution" in Europe, 1500–1800 (Hall 1956).
3. The investigations performed in a given organization such as Leiden University (Ruestow 1973) or Bell Laboratories (Smits 1985).
4. The effects of society and culture on science, sometimes construed as the social construction of science (e.g., Barnes, Bloor, and Henry 1996; Golinski 1998).
5. History of a discovery, as in determining the structure of DNA (Watson 1968).
6. History of a discipline such as chemistry (Partington 1961–1970).
7. The organization of science, such as the history of eighteenth-century scientific societies (McClellan 1985) or the growth of "big science" (Galison and Hevly 1992).
8. Biography or autobiography of an investigator; e.g., Powers' (2012) biography of chemist Herman Boerhaave.
9. Study of a specific apparatus (or instrument) and classes of them, as in Hackmann's (1978) monograph on eighteenth-century electrical machines.
10. Researching a specific and usually large-scale undertaking, such as the Manhattan Project (McKay 1984).
11. Ethnography of a laboratory, as in Latour and Woolgar's (1979) fieldwork at the Salk Institute.
12. The cognitive structure of scientific knowledge, such as Nagel's (1961) philosophical treatment.
13. Cognitive processes of scientific research, as in the study of discovery by Klahr et al. (2000).
14. The scientist as a goal-seeking social being (Osbeck et al. 2011)
15. Science and politics; science and government (Hughes 2002).

Researchers in a specific discipline tend to emphasize particular perspectives. Philosophers study research processes and the structure of knowledge. Sociologists treat scientific research as collective action and illuminate the role of social processes and consensus building in the evaluation of knowledge claims. Thus, the sociology of science includes the ethnography of a laboratory, the organization and social construction of science, and science and politics. Employing an ethnographic perspective, sociocultural anthropologists delve into the nature, organization, and uses of scientific knowledge in traditional societies and also research the role of culture in the everyday practice of science in industrial societies. And psychologists conduct experiments on the cognitive processes of learning and discovery. In contrast to social and behavioral scientists, a physicist or chemist writing about her discipline may adopt a biographical or autobiographical perspective as well as histories of ideas, disciplines, and discoveries. The works of historians range widely over many perspectives. Despite the intellectual faddism endemic in the social sciences and humanities, rendering some perspectives more or less trendy at given times, all perspectives survive in the academy today.

Archaeologists occupy a large—and growing—niche in the study of science. Simply put, this book is primarily about defining and illustrating that niche, highlighting the contributions that we have made and may make. The archaeology of science consists of perspectives that *in their entirety* define a distinctive approach. Individually, the perspectives are not exclusive to archaeology, nor would they all be present in any one study, but together they distinguish a diversified research program that is illuminating some dark corners of science.

The Archaeology of Science

Robert T. Gunther used the phrase "archaeology of science" to label his efforts, in the early decades of the twentieth century, to build science museums at Oxford and Cambridge Universities (Bennett 1997). Archaeology was in some ways an apt term for digging into historical documents and for assembling the scattered and tattered apparatus of earlier British science. Anderson (2000), in a paper advocating the study of surviving chemical apparatus together with documents, titled his paper "The Archaeology of Chemistry." Although these scholars construe the archaeology of science somewhat narrowly, both fasten on the foundation of any archaeological investigation: a concern with people making and using artifacts.

Artifacts, also known as technologies, material culture, products, objects, devices, gadgets and gizmos, or just plain things, encompass everything that people make or modify. The artifacts of scientific activities, especially those participating in experiments, are often called *apparatus* (a term that is both singular and plural) or, in modern science, instruments (Baird 2004; Bud and Warner 1998).

Artifacts are central to archaeological research, but we do not consider them in isolation. Rather, our task is to situate artifacts, whether ancient or modern, in their behavioral, societal, and environmental contexts, making use of all relevant lines of evidence. Thus, depending on our questions, we may exploit the archaeological record, the historical record, the ethnographic and ethnohistorical records, and oral history. We might even conduct experiments, carry out an ethnoarchaeology project, make use of knowledge and technology from the physical and biological sciences (archaeometry), and scour the literatures of other disciplines. Archaeologists are eclectic and opportunistic, reaching across subject matters and respecting no disciplinary boundaries. And so it is, especially, in the archaeology of science.

In addition to placing artifacts into their contemporaneous contexts, we order them temporally and ask about the hows and whys of technological change (Schiffer 2011). Adopting a temporal perspective leads to new insights as we document and explain changes in apparatus and illuminate their roles in creating knowledge (e.g., Schiffer 2008; Schiffer, Hollenback, and Bell 2003). We also do comparative studies, seeking to explain variability among artifacts, even those in different technological traditions.

The proper archaeology of science has a long history that began by the early nineteenth century. The initial stirrings of interest appeared when researchers asked

how certain enigmatic artifacts of prehistory were made. In seeking answers, often through replicative or imitative experiments, archaeologists modeled the generalizations implicit in ancient activities such as the manufacture of metal, ceramic, bone, and chipped-stone tools. Because only archaeology can reconstruct the science of prehistoric societies, such research continues to be of great interest and is now so highly developed that syntheses fill many books and monographs (e.g., Adams 2002; Griffitts 2006; Rye 1981; Whittaker 1994).

During the twentieth century, archaeologists also investigated the ancient counterparts of modern science and engineering. A well-known example is archaeoastronomy, the reconstruction of a society's knowledge about the motions of the sun, moon, planets, and stars (for a recent synthesis, see Milbrath 2009). By relating ethnographic and experimental evidence to archaeological facilities and structures, archaeologists can sometimes infer how a technology enabled a group to predict the seasonal events affecting its subsistence and ceremonial activities. There are also continuing studies on how people built large-scale "public works" such as the Egyptian pyramids, Stonehenge, and the Hohokam canal system of southern Arizona. Often framed as inquiries into ancient engineering practice, these studies sometimes bring to light the underlying science.

Recently, archaeologists have embraced the remains of early modern (ca. 1600–1800 C.E.) and modern science (post-1800 C.E.). Employing a wide range of data-gathering techniques, we have reported on subjects as diverse as medical instruction and practice (Henderson, Collard, and Johnstone 1996; Hull et al. 2003; Veit 1996), Cold War facilities in the former Soviet Union (Fowler 2008), manufacture of military explosives (Cocroft 2000), refuse from Newton's alchemical experiments at Cambridge University (Spargo 2005), astronomical observatories (Bickford 2011; Edmonds 2010; Evans and Newman 2011), the Roswell, New Mexico "alien" crash site (McAvennie 2004), Darwin's archaeology of worms (Evans 2009), electrical technologies (Schiffer 2008; Schiffer, Hollenback, and Bell 2003), and other subjects as discussed in later chapters.

Many projects on early modern and modern science are done as cultural resource management (CRM) or heritage studies. In the USA most CRM research is reported in difficult-to-access "gray" literature. Nonetheless, by obtaining many obscure reports, I have learned that this research contributes much to the study of science by documenting and contextualizing the material remains of significant and sometimes formerly secret activities. Having management goals and budgets tailored to those goals, CRM projects seldom allow archaeologists to realize the full research potential of the material record. And so this book presents several case studies, drawn largely from the gray literature of the USA, that describe previous research and highlight future opportunities (see chapters in Part III).

By way of concluding this section, I suggest that the following perspectives characterize the archaeology of science.

1. Studying scientific activity in any society, whether prehistoric, historic, or modern.
2. Asking any question that focuses on the interactions of people, artifacts, and natural phenomena in scientific activities.

3. Placing the activities and artifacts of science in their behavioral, societal, and environmental contexts.
4. Modeling the scientific knowledge embodied in any activity.
5. Explaining change and variability in the design of artifacts used in scientific activities.
6. Crafting generalizations through comparative research.
7. Drawing on diverse lines of evidence from the historical and archaeological records as well as oral history, experimental archaeology, ethnoarchaeology, and archaeometry.

A Preview

This book consists of three parts. Part I sets forth the book's orientation (in "Introduction") and lays the conceptual foundation for the case studies. "Science: A Behavioral Perspective" introduces the basic concepts of behavioral archaeology that enable me to explore the relationships between science and technology. In "Varieties of Scientific Knowledge" I discuss in behavioral terms the major kinds of scientific knowledge, emphasizing their predictive capabilities. Recipes are shown to be an important and universal kind of scientific knowledge.

Part II highlights the contributions of experimental archaeology, ethnoarchaeology, and archaeometry. The case studies of experimental archaeology (in "Contributions of Experimental Archaeology") are (1) the replication of Folsom points in search of manufacture recipes and (2) controlled experiments that yield generalizations about the effects of surface treatments on the performance characteristics of traditional ceramic cooking pots. In "Contributions of Ethnoarchaeology," case studies on the manufacture of alabaster vessels in Egypt and on the use of hide scrapers in Ethiopia indicate how ethnoarchaeologists may furnish behavioral data for modeling the science embedded in traditional technologies. I also present a section on the contributions of controlled comparisons in ethnoarchaeology. Two case studies in "Contributions of Archaeometry"—one on the pigment known as Maya blue, the other on cylinder jars from Chaco Canyon, New Mexico—demonstrate that archaeometric research creates *new* evidence for modeling past scientific knowledge.

Part III contains extended case studies that illustrate the potential held by the archaeological and historical records for wide-ranging research on scientific activities. "The Apparatus of Modern and Early Modern Science" engages the artifacts of early modern and modern science, employing examples that build on my previous studies of electrical technologies.

Thomas Edison's new technologies required that he create new science. In "Thomas Edison's Science" I summarize the CRM studies that have taken place at his "invention factories" in Menlo Park and West Orange, both in New Jersey. And, on the basis of archival materials, I examine one of Edison's most successful technologies—the nickel-iron storage battery—to illuminate his strategy for testing recipes.

"Exploration and Colonization" treats studies of colonization and exploration, which have been exceedingly fertile contexts for fashioning new science. Two case studies, preceded by a general discussion of adaptive scenarios, are provided: (1) the Polynesian colonization of New Zealand and (2) the English exploration and settlement of Roanoke Island in North Carolina.

Antarctica is one of the most challenging places to do archaeological fieldwork, yet some has been done. "Scientific Expeditions to Antarctica" identifies research possibilities, including those afforded by extensive databases already available on the stations established by many countries. I also discuss fieldwork carried out at East Base, the first permanent US station.

From the Manhattan Project to the cessation of nuclear weapons testing (and beyond), the US nuclear establishment left abundant remains of research and development activities. "The US Nuclear Establishment" summarizes several archaeological and historical CRM surveys conducted at Las Alamos, New Mexico, and the Nevada National Security Site. I discuss at some length Project Rover, which developed a nuclear–thermal engine for spacecraft.

"Archaeology of the Space Age" treats the archaeology of aerospace activities, calling attention to the remains of space exploration on Earth and elsewhere in the solar system. I highlight the research potential of the historical and archaeological records of US bases where missile and rocket tests have taken place, focusing on the White Sands Missile Range in New Mexico, where German V-2 rockets were tested immediately after World War II ended.

"Discovery Processes: Trial Models" explores the potential of an apparatus-centered approach to yield generalizations about discovery processes, which have received relatively little attention in science studies. Several provisional models of discovery processes are presented.

Acknowledgments I thank Lawrence H. Lazarus for comments on this chapter.

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Science: A Behavioral Perspective

The "archaeology of science" is defined here as *archaeological research into the processes and products of science, which includes the scientific activities of any person, organization, or society as well as the comparative study of such activities*. By "archaeological" is meant studies that focus on people–artifact interactions. This framing is general enough to include the many perspectives listed in the "Introduction." The archaeology of science should not be confused with science in archaeology (i.e., archaeometry) or archaeology as a science.

Behavioral Archaeology: An Introduction

Because behavioral archaeology foregrounds materiality (Schiffer 2010; Skibo and Schiffer 2008), it is an appropriate conceptual scheme for research on the artifact-intensive activities of science. This section introduces elements of behavioral archaeology that have guided my construction of, and case studies in, the archaeology of science.

Originating at the University of Arizona in the early 1970s, behavioral archaeology has been elaborated in many places. In their first programmatic statement on behavioral archaeology, Reid, Schiffer, and Rathje (1975:864) defined archaeology "as the study of relationships between human behavior and material culture in all times and all places." Although now widely accepted, this expansive definition was regarded as radical during the mid-1970s, for it was a mandate to study everything from yesterday's garbage to portable radios to modern scientific apparatus. Although useful, this definition implies that artifacts are separate from, not an integral part of, human behavior. However, if human behavior consists of activities, and artifacts take part in virtually all activities, then human behavior has no existence apart from the artifacts that it includes (Walker, Skibo, and Nielsen 1995). Accordingly, behavioralists now define archaeology as the study of interactions between people and artifacts in activities, whenever and wherever they take place (e.g., Schiffer 2010,

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2011; Schiffer and Miller 1999; Skibo and Schiffer 2008). The societal and environmental contexts of activities are also of interest and take on causal importance in many studies.

Interactions, Interaction Modes, and Performance Characteristics[1]

An activity's forward motion is impelled by the sequence of interactions among its *interactors*, which may include people, artifacts, and environmental phenomena. There are five major interaction modes: mechanical, chemical, thermal, electrical, and electromagnetic. Mechanical interactions entail physical contact; in chemical interactions there are reactions; during electrical interactions a flow of electrons or other charge carriers takes place; in a thermal interaction one body heats or cools another; and the electromagnetic mode takes in electromagnetic radiation such as light or radio waves.

Some interactions involve more than one mode. Thus, chewing a piece of bread—so obviously mechanical—also consists of chemical reactions between the masticated bread and salivary enzymes, as well as among the bread molecules and taste buds and olfactory receptors. Virtually any mechanical interaction involving a sighted person also includes electromagnetic interactions (as light). The possibilities for combined interactions are endless. Further complexity arises because most activities are composed of varied interactions occurring simultaneously and sequentially. So as not to overlook essential contributions to an activity's forward motion, we ask the following question: What are the activity's participating interactors and constituent interactions?

For a specific interaction to take place, each participating interactor must carry out one or more *performances*. As an example, let us take the cooking of stew in a ceramic pot on a hearth. To keep it simple, I omit the cook and light source; thus, the relevant interactors are stew, pot, and hearth. This activity requires the following performances: the hearth transfers heat to the pot; the pot contains the stew, becomes hot from the hearth, and conducts heat to its contents; and the stew gradually cooks.

In order to carry out its functions competently—i.e., meet an activity's performance requirements—an interactor must possess relevant performance characteristics. *A performance characteristic is a capability, competence, or skill that may be exercised (or come into play) in a given interaction.* Let us illustrate the concept of performance characteristic by revisiting the stew. To serve as a heat source, the hearth must reach a specific temperature in a timely manner and furnish the heat somewhat continuously. The cooking pot must possess ample resistance to thermal shock and thermal spalling, the ability to rest on the hearth without tipping or deforming, and adequate heating effectiveness (Schiffer and Skibo 1997). And after

[1] This section draws, often verbatim, on Schiffer (2011, 25–28).

heating, the stew has to achieve palatability. By virtue of these performance characteristics, the hearth, pot, and stew all interact competently, which satisfies the performance requirements, and so the activity moves forward. Every activity can be specified in terms of its interactors' *performance requirements*.

Adding the cook to this example enables us to consider how people can carry out interactions competently. Bundled into a person's interaction-specific performance characteristics is tacit knowledge, especially skill. Through experience, for example, the cook learns how to maintain the fire at the proper heat and to recognize when the stew is palatable. Perhaps we should think of skills as micro-performance characteristics. Studying the acquisition of skill is a crucial research area in archaeology, but it is assumed in many behavioral studies that the people have acquired the skills needed for a given activity.

Throughout an artifact's life history, performance requirements change from activity to activity. During manufacture a piece of chert or flint should have good flakeability so that it can be knapped into a knife, but during use the knife should be able to cut cleanly. Consequently, as an artifact passes from activity to activity during its life history, different performance characteristics come into play.

Performance characteristics of artifacts are commonly confused with a related concept, material property (Schiffer 2003). *A material property is a measurable quality of a material*, such as the tensile strength of flint, the Mohs hardness of porcelain, and the color of sugar, which is usually assessed in relation to a standard scale, on a specimen of particular size and shape, under specific laboratory conditions. In contrast, a performance characteristic is a *behavioral capability defined with respect to actual interactors taking part in real-world activities*. I emphasize that an artifact's performance characteristics are not material properties, much less essentialist qualities, but are constructs defined in relation to a specific interactional context. However, material properties are among the factors that influence performance characteristics. Thus, tensile strength affects the flakeability of flint. Some performance characteristics are general, in that they may come into play in varied interactions, such as the scuff-resistant shoe that can resist scuffing in a host of contacts.

Behavioralists also specify families of *sensory performance characteristics*, which depend on the human senses of sight, touch, hearing, smell, and taste. A sensory performance characteristic pertains to any person, artifact, or environmental phenomenon *in relation to its interaction with a person*. Enabling many artifact functions, sensory performance characteristics speak directly to a person's immediate experience of something, such as the palatability of stew.

During more than three decades of building and using behavioral models, we have continuously expanded the definition of performance characteristic, adding even financial interactions, as in an artifact's affordability. We may also treat organizations as macro-interactors and specify their performance characteristics; after all, households, partnerships, and corporations have different capabilities that come into play in internal and external interactions. Today, *performance characteristics denote varied competences, capabilities, and skills that enable virtually any kind of performance by any kind of interactor*.

Behavioral Chain, Technical Choice, and Effect

A *behavioral chain* is the entire sequence of activities in any interactor's life history (Schiffer 1975), and applies equally to a singular artifact, such as Renoir's painting, *Luncheon of the Boating Party*; craft items, such as the rice-cooking pots made by all potters in Dangtalan, a village in the Philippines; and mass-produced items, such as a Hershey's milk chocolate bar with almonds.

During the portion of a behavioral chain that encompasses the procurement of materials and manufacturing process, artisans or designers make *technical choices*, selecting activities and interactions from among available alternatives (Schiffer and Skibo 1987, 1997). In forming a ceramic bowl, the modern potter chooses a clay, the kind and size of temper (if any), moisture content, forming process (e.g., hand-building, coiling, fast wheel, molding, or slab), and so forth. These choices depend on the potter's skills as well as expectations about how the clay, and then paste, will interact with her hands and tools. And those expectations, in turn, are based on a host of descriptions and generalizations that the potter has acquired, often implicitly, in the course of learning to make pots while participating in a community of practice such as a village's potters.

Sometimes the potter makes technical choices that depart from tradition, such as trying out a different clay or tempering material to learn its effects. Indeed, changing an interactor or performance in an activity may result in a novel interaction. In adding for the first time coarse sand to the clay, the potter on a fast wheel observes that the paste's rough texture makes it difficult for her hands to shape it into a vessel. Regardless of the outcome, in changing an activity's interactors or performances the potter has become an experimenter—no different from a modern physicist or chemist or biologist.

In general, a novel interaction or performance may be perceived by the experimenter and ascribed significance; in such cases, it becomes an *effect* that can be described or generalized. Thus, the potter may assume that any coarse-textured paste is unsuitable for throwing on the fast wheel. The experimenter may go further, claiming a discovery by communicating that effect to others through word of mouth, demonstration, publication, and so forth (Shapin and Schaffer 1985). In books on craft pottery, one can find the generalization that much coarse temper has dire effects on paste workability for fast-wheel work.

Science scholars have shown that a discovery claim becomes part of a tradition only after evaluation and acceptance by the relevant social group—i.e., a community of practice such as potters, astronomers, or factory workers (e.g., Barnes, Bloor, and Henry 1996). Discovery claims may be rejected during the evaluation process, especially if they are at odds with an accepted theory (Kuhn 1970). In 1989 chemists Stanley Pons and Martin Fleischmann claimed to have discovered a table-top, electrochemical method for producing nuclear fusion ("cold fusion"). Even before numerous experiments by other investigators failed to replicate their findings, nuclear physicists—the relevant community of practice—dismissed cold fusion because it contradicted foundational theory. However, sometimes even a foundational theory may have to be revised or replaced.

What Is Science?

As a behavioral definition of science, I offer the following: science consists of diverse "discovery" and other processes for creating shared knowledge—implicit or explicit—that can be employed to anticipate specific empirical phenomena of the material world. Scientific knowledge, as descriptions and generalizations, deals with static and dynamic patterns. Let us now unpack this definition.

1. "Diverse 'discovery' processes." "Discovery" is in quotation marks because every discovery begins as an invention, the creative act of assigning significance to—and usually labeling as something new—an environmental or culturally-created phenomenon or effect. Processes leading to a discovery claim include observations of environmental phenomena, trial-and-error, hypothesis testing, structured experiments, computer simulations, cogitation, and so forth (for examples, see "Discovery Processes: Trial Models"). People in all societies employ discovery processes but the mix varies: in traditional societies people observe environmental phenomena and use several modes of experimentation, particularly trial and error; people in industrial societies use the entire range of discovery processes. The most important "other" processes of science are *communication*, the transmission of a discovery claim to others; and *evaluation*, the assessment of a discovery claim by the pertinent community of practice.
2. "Shared knowledge." This is equated with descriptions and generalizations of several kinds, which may be firmly established and long-lived or poorly supported and transitory. Descriptions include singular observations as well as categories and classifications; generalizations include empirical generalizations, experimental laws, recipes, theories, and models (see "Varieties of Scientific Knowledge"), which vary in generality, degree of abstraction, complexity, and empirical content. Whether the knowledge is explicit as in much modern science or implicit as in many traditional societies, the product of science is "shared" knowledge (Gooding 1990; Shapin 1996:106). But, I hasten to add, sharing may be limited to a very small group. Communication processes, including person-to-person speech, learning by watching and doing in a master-apprentice setting, copying of ancient manuscripts, books and journals, and electronic mass media, bring about the sharing of descriptions and generalizations. Skill is acquired mainly through "hands-on" experience.
3. "Can be employed to anticipate specific empirical phenomena of the material world." "Empirical phenomena" refer to an observable or potentially observable performance of any interactor, whether cultural or environmental. Performances may be observed by human senses unaided or mediated by apparatus (Rothbart 2007; Schiffer and Miller 1999). By "anticipate" is meant expecting, forecasting, or predicting a performance not yet observed. For various reasons predictions are not necessarily realized in practice. The term "specific" rules out blanket expectations derived from religious or other ideology-based beliefs. A St. Christopher's medal, for example, leads a believer to expect safe travel but does not protect that person from a specific danger on a given journey. "Material world" includes artifacts and both living and nonliving phenomena of the natural environment.

4. "Static and dynamic patterns." A pattern is a regular or repetitive phenomenon. A static pattern is relatively constant such as the height of Mt. Whitney in California, the classification of Pleistocene megafauna, and anatomical drawings of a normal human body. A dynamic pattern includes changes brought about by cultural or noncultural processes, such as the smelting of iron or the ontogeny of an antelope embryo. Patterns may be represented by any descriptive system, including words, drawings, photographs, equations, and mechanical and computer models.

In short, science consists of the kinds of knowledge that make possible, through prediction, potentially effective interactions with artifacts as well as entities in the natural environment. Prediction looms large in everyday activities and, I suggest, is in many societal contexts the principal incentive to create new science.

The Science Project

An important analytical unit for research in the archaeology of science is the *science project*. A set of related activities taking place under the supervision or direction of an investigator, a science project *may* result in new descriptions and generalizations. Because science projects are universal, we may focus on those occurring at any time in any society, regardless of the contexts in which they arise. Indeed, virtually every societal context foments projects. Einstein, who developed his Special Theory of Relativity as a Swiss patent examiner, was engaged in a project to determine the contemporaneity of distant events, prompted by societal demands to coordinate time zones and train schedules (Galison 2003).

Projects have outcomes (also termed results or findings), which may or may not accord with the investigator's expectations. An outcome may be merely "the appearance of a new effect" or a specific prediction verified to five decimal places. Although a completed project has an *outcome*, its significance in the short or long term has no bearing on its potential to arouse archaeological interest. We may study projects whose results were the following: (1) negative, inconclusive, or highly controversial, (2) regarded as trivial, and so disappeared into obscurity, (3) incorporated seamlessly into daily practice, or (4) judged important and adopted far and wide.

The project orientation obviates the need to identify a "scientist," for we are interested in all people, regardless of occupation or social position, who take part in scientific activities. Thus, we include nonliterate people who left only an archaeological record or who whose written record was produced by ethnohistorians or ethnographers. Also, because the term scientist was coined only in the 1830s, applying it to a traditional society or to anyone in the West before about A.D. 1600 is inappropriate. For ease of communication, let us apply the term "investigator" to the person(s) who initiates, carries out, or reports a project's outcome.

The simplest science project consists of one person conducting few activities with scant apparatus, such as observing and taking notes about a plant or animal, rock or

river, tides or sunrise. At the other extreme is the International Space Station, one of the world's largest, most complex, and most expensive apparatus, which was designed, built, and operated by thousands of people from a consortium of countries.

A science project may have several stages, perhaps with different investigators employing different apparatus. For example, the seventeenth-century experiment at Puy de Dome, which showed that a vacuum could exist and that air has weight, was conceived by Mersenne, initiated by Pascal, carried out by Perier, and reported and defended by Pascal (Shapin 1996:41–42). When warranted, we may bundle together apparently separate projects. Thus, it might be instructive to include as the first stage of the Puy de Dome project Torricelli's demonstration of the vacuum inside the top of a closed mercury tube, for that experiment led directly to the others. There is great flexibility in how we draw a project's behavioral boundaries around investigators, sequences of activities, apparatus, and places.

Developmental Distance and Resource Needs

The entirety of activities and resources required by a science project is its *developmental distance*, a concept borrowed from the study of technology (Schiffer 2011:86–89). A science project's developmental distance may be difficult to estimate at the outset, especially if it is vast. CERN's Large Hadron Collider has had enormous resource needs, including 1,600 superconducting electromagnets and 96 tons of helium to cool them, three huge detectors, the aggregate knowledge of physics and other sciences possessed by its thousands of collaborators, numerous engineers and technicians, a hierarchical organization, an enormous underground laboratory, and financial contributions of many countries. And its data stream, generated by collisions of subatomic particles traveling near light speed, is analyzed by tens of thousands of computers worldwide.[2] As of late 2011, the project had spent more than $10 billion, several times the original estimate.[3] Some developmental distances are so daunting that a project may be terminated when resource limitations are reached—even if sponsored by a government. In the USA, construction of the Superconducting Supercollider was well underway when cost overruns led Congress to end funding.

In addition to the findings of related projects, a science project may require any of the following resources[4]:

1. *Human resources* are people having relevant experience, knowledge, skill, and access to social networks.
2. *Organizational resources* are the groups for enabling and carrying out the project, such as a family, attached specialist, work group, and ceremonial

[2] http://press.web.cern.ch/public/en/LHC/LHC-en.html, accessed 16 November 2011.
[3] http://en.wikipedia.org/wiki/Large_Hadron_Collider, accessed 16 November 2011.
[4] Adapted almost verbatim from Schiffer (2011, 86–88).

society; industrial, national, and university laboratories; and partnership, corporation, factory, and government agency.

3. *Technological resources* are materials, components, products (including tools and machines), processes, facilities, and structures.
4. *Energy resources* include animals, people, sunlight, wind, flowing water, wood, coal, and other hydrocarbon fuels.
5. *Utility resources* are electricity, gas, water, waste disposal, and so on.
6. *Information and communication resources* are books, journals, telegraph, telephone, and the Internet; methods of accounting and record keeping from cuneiform tablets to mainframe computers to people holding institutional memories; and social networks that furnish information about other resources.
7. *Linguistic resources* make it possible to describe a project and its outcome, to learn about resources described in other languages, and to communicate with varied groups: participants, supporters, other investigators, a larger community.
8. *Ideological resources* justify a project on the basis of intellectual, religious, moral, community, national, or other values.
9. *Transportation resources*, from foot travel to cargo planes, move other resources to where they are needed.
10. *Locational resources* are tied to a place but do not fit easily into other resource categories. They may be environmental, such as land for a building or a secluded place for a secret project; or cultural, such as an abandoned mine to accommodate a detector of subatomic particles.
11. *Legal and political* resources include patents, licenses, permits, and contracts; the ability to form companies or other organizational units; endorsement by political leadership; and enabling legislation. Resources may be sought by inquiry, application, friendly persuasion, and lobbying.
12. *Financial resources* enable acquisition of many other resources and may range from the investigator's personal savings to sponsorship by a company, university, or government.
13. *Time* is needed to span a project's developmental distance. Some projects have an open-ended time frame, many require more time than initially estimated, and others struggle under an inflexible contract deadline.

Several resource categories have overlapping memberships, such as the people in human, organizational, information, and energy resources. In each category, however, people play different roles: furnishing expertise, managing a project, providing information, and supplying labor, respectively. The emphasis here is on delineating a project's resources on the basis of their required performances and interactions.

At any given time, a society's resources determine the kinds of projects that appear to be immediately feasible. In mobile hunter–gatherer societies, scant resources of nearly every kind restricted projects to those having very modest developmental distances, and so people created mainly descriptions and generalizations necessary for daily activities with discovery claims usually made and evaluated by a small local group. People in tribal agricultural and pastoral societies had different problems to solve as well as access to a larger and more diverse resource pool, and

so could take on projects having a greater developmental distance. The latter projects included fashioning generalizations needed for growing and processing crops, animal husbandry, and building irrigation systems. With the advent of chiefdoms and states, leaders could draw on a greater resource pool for tackling still more ambitious projects. Institutions attached to states, such as an established church, could harvest resources from laypeople and invest them in projects such as building an astronomical observatory. States also make possible science set in colleges and universities, national laboratories and research institutes, corporations, and various governmental agencies. It should be noted, however, that resource limitations do not prevent the smallest societies and organizations from generating theories.

In resource-rich states, a variety of projects with enormous developmental distances may be feasible in principle but only some are undertaken. Indeed, decision-making groups deny resources for some projects but lavish them on others, dictating the research problems, sometimes in great detail. We might want to study, comparatively, how decision makers set the priorities for supporting projects.

Science Projects and Technology Projects

Some science projects can use off-the-shelf technologies, but others require new apparatus. In the latter case, the investigator initiates one or more *subsidiary projects* to develop technologies having the necessary performance characteristics. Subsidiary technology projects are exceedingly common in the science of industrial societies. The most ambitious science projects, such as the Hubble Space Telescope and the Large Hadron Collider, required many thousands of new artifacts whose development consumed the bulk of financial resources. Even in earlier times many a chemistry experiment required the investigator to make or commission a *new* variety of glassware or furnace. Likewise, in traditional agricultural societies, projects to observe solstices sometimes necessitated new structures.

According to historian Layton (1971), science and technology are mirror–image twins (see also discussions in Garber 1990; Molella and Reingold 1973; Roller 1971). Thus, *not only may science projects generate subsidiary technology projects, but technology projects may also generate subsidiary science projects*. A notorious case of the latter is the Manhattan Project, which produced the world's first atomic bombs. At the project's start, nuclear science was in its infancy and lacked much essential knowledge. To fill the voids many scientists, including illustrious physicists, pursued thousands of subsidiary science projects at Los Alamos and elsewhere (see chapter "The US Nuclear Establishment"). Like the Manhattan project, technology projects with a lengthy developmental distance produce a *discovery cascade*—the continuous outpouring of new science. But new science also issues from less resource-intensive projects, even in traditional societies. The first developers of pottery had to learn about the workability of local clays as well as the effects of temper on paste-drying rate and thermal shock resistance (Schiffer and Skibo 1997). Likewise, in making a bow the first archers had to discover the properties of

different woods and fibers, and in using the bow they also created knowledge of ballistics. Moreover, the creators of atomic bombs, pottery, and bows and arrows—and of all other new technologies—had to fashion effective recipes for manufacture processes.

Large-scale projects of science or technology tend to generate a plethora of *both* knowledge and artifacts through nested subsidiary projects. Let us take a familiar example, President Kennedy's project to put men on the Moon by the end of the 1960s. To achieve that technological outcome required herculean projects that created, for example, new science about the Earth, Moon, and outer space. Those subsidiary science projects in turn required technology projects to develop rockets, satellites, probes, instruments, and so forth, which spun off still more science projects (see chapter "Archaeology of the Space Age"). Clearly, the nesting of science projects in technology projects and vice versa, seemingly *ad infinitum*, has contributed in modern times to an interdigitation of scientific and technological activities in practice. It would be instructive to create a model of the life history of a large-scale science or technology project that illustrates the ramifying pattern of subsidiary projects. And several such models would invite comparative analysis.

In the real world of mega projects, such as the Moon landing (new technology expected) or the Large Hadron Collider (new scientific knowledge expected), scientists and engineers work together on subsidiary projects and sometimes reverse roles or play both roles. Although they may participate in the same project, scientists and engineers remain members of separate professions (i.e., communities of practice), science and engineering disciplines retain their distinctiveness in the academy, and both kinds of projects remain distinguishable. Yet, in publicizing mega projects, the media and popular culture have blurred the distinction between science and technology, for both may generate projects that produce knowledge and technology in profusion.

Why Do People Undertake Science Projects?

Among the many factors offered to explain the initiation of science projects curiosity looms large. Blackwell (1969:130) put this view succinctly, "an aroused curiosity is the driving force behind inquiry" (see also Gruender 1971). More recently, Hoffmann (2011) insisted that projects arise when an investigator finds something "interesting." What makes something interesting? He mentions an exciting possibility, an unexplained phenomenon, an anomaly, a puzzle. These are all plausible paths to science projects, and examples of them abound, especially in *investigator-originated* projects in universities. However, that an investigator fastens on something interesting is neither a necessary nor a sufficient condition for starting a project. Even so, I do not go so far as Smith's (1971:147–148) claim that "discovery of new effects inspired only by curiosity is by its very nature rare."

In modern times, especially, many science projects are *sponsor-originated*: a government agency, organization, or corporation specifies a project it will fund

because the outcome is expected to enhance war-making capabilities, public health, product development, and so forth. The outside investigator who signs on for the project has a financial stake in doing the research but may have little intellectual interest. Likewise, employees of corporations and governmental agencies are often assigned projects in which, at least initially, their interest may be slight. Also, sovereigns and superrich people sponsor projects and hire people to carry them out. In sponsor-originated science projects, investigators and sponsors may have different motivations, and intellectual curiosity may not be important for either group.

Beginning in early modern times, an investigator who solved a widely recognized problem, such as determining longitude at sea or making a long-lasting battery, might receive a prize, government pension, prestigious position, or peer recognition. *Reward-oriented* projects are common today, and the range of rewards is large. Watson and Crick could have attempted to solve the structure of any number of organic molecules, but they chose DNA, correctly anticipating that the solution would garner for them a Nobel Prize (Watson 1968). People tend to flock to a "hot" area, such as high-temperature superconductors, carbon nanotubes, and quantum computing, because a major discovery may lead to prestige, grants, prizes, lucrative employment, perhaps even a fortune if the discovery—perhaps a recipe—can be patented and sold or licensed. How else do we explain why many investigators set aside an ongoing project in favor of one that is trendier? Intellectual interest of course may grow rapidly in reward-oriented projects owing to the intriguing problems they present.

On the basis of discussions earlier in this chapter, I suggest that the single greatest spur to science projects—in the past and present—is technology projects, which produce recipes and also may generate subsidiary science projects. Technology projects, whether in prehistory or the present, do not end with the development of the technology itself, for additional generalizations are needed to permit its competent operation, maintenance, and perhaps reuse or discard. To this day, investigators at Los Alamos National Laboratory study the deterioration of America's nuclear weapons, creating new science as needed; and the search for a permanent repository for nuclear waste has generated many projects in geology and materials science.

Regardless of the sponsor's or investigator's motivations, many a science project begins with a question or a problem. To wit, something is problematized with the expectation that a project may provide a solution. Problems and questions are equivalent ways of framing a project's starting point, and both specify the scientific knowledge being sought. When a question or problem is implicit in a past project, we may model it. In the chapter "Discovery Processes: Trial Models," I discuss other ways that projects begin.

I emphasize that new scientific knowledge may arise *in any societal context* when people develop a new technology. In the past, worshiping gods, crossing the seas, hunting large game, gathering root vegetables, and irrigating a field all inspired new technologies whose subsidiary projects created new science. This perspective gives us a mandate to document or infer the science generated during any technology's life history. Let us not forget that technological activities of every kind are knowledge-intensive (Layton 1974; Schiffer 1992, 2011).

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Varieties of Scientific Knowledge

Many science scholars emphasize the kinds of knowledge that make possible explanation. However, I regard explanation as secondary to prediction because virtually every human activity in every society moves forward on the basis of the predictions that scientific knowledge enables (cf. Reichenbach 1966; Schiffer and Miller 1999). Accordingly, this chapter shows, through definitions, examples, and discussions, the predictive capabilities of the several major varieties of scientific knowledge—*as defined behaviorally*.

Scientific knowledge consists of two major domains: descriptions (observations, categories, and classifications) and generalizations (empirical generalizations, experimental laws, recipes, theories, and models). *As cognitive structures, descriptions and generalizations have to be modeled by the archaeologist on the basis of behavioral and material evidence*. The modeling process allows us to study the scientific knowledge of nonliterate societies as well as the implicit knowledge found even in modern science.

In the actual conduct of science, the varieties of knowledge are interdependent. Thus, one can neither define an observation without mentioning categories nor discuss categories without referring to empirical generalizations. And all generalizations necessarily incorporate categories and descriptions.

Descriptions

Observations

An observation is an investigator's account of a *singular* performance or interaction, which may be expressed, for example, as a proposition or image (on the construction of observations, see Gooding 1990). In order for a performance to be observed

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it must be registered in the brain as a sense impression.[1] From vision to smell, an interactor's sensory performance(s) may stimulate neurons and be registered. A person assigns meaning to registered observations by placing them in linguistic categories: a low rumbling sound is a *distant train*; peeps and chirps are *bird calls*. The investigator may invent a category to describe a new observation, but eventually that category must be shared—i.e., become mutually intelligible to the members of a community of practice. Thus, "Observation implicates the natural as well as the social world" (Gooding 1990b:76; see also Barnes, Bloor, and Henry 1996).

In modern science, especially, investigators observe not the performance of an interactor itself but that of a mediating apparatus. Radio waves, exoplanets, and atoms are far beyond our sensory capabilities, but investigators have built radio receivers, powerful telescopes, and scanning tunneling microscopes that enable humans to register the performances of such phenomena. The apparatus selects the performance and transforms it into a mode that is human-observable—usually visual. *Mediated observations* are subject to revision because an apparatus may be malfunctioning, imprecise, or built on the basis of erroneous theories. In any of these scenarios, another apparatus may perform differently and yield new observations. In traditional societies and in the earliest exploring expeditions, some observations may have been made with naked human senses.

Investigators also build apparatus that create their own performances. Foucault's pendulum, whose low-friction suspension allowed the plane of its swing to rotate, described a circle over a 24-h period, a stunning performance that convinced many skeptics that Earth revolved daily on its axis (Aczel 2003). Some apparatus give rise to phenomena that can be observed nowhere else on Earth (cf. Price 1984): a smelter produces molten copper, a nuclear reactor makes plutonium 239, a laser generates a beam of coherent light. Such performances are as much the result of cultural as of "natural" phenomena (cf. Edgeworth 2012).

Generalizations "built into" an apparatus determine the categories an investigator applies to its performance, and thus its interpretation. The operation of a simple wattmeter depends on the effect first reported and generalized by Oersted in 1820: the flow of current in a conductor creates magnetism in its immediate vicinity (Dibner 1961). A simple wattmeter measures the strength of magnetism next to the conductor, which causes an indicator needle to deflect in proportion to the amount of current flowing. This crucial effect, *built into the wattmeter's operation*, permits the investigator to assign meaning to the device's visual performance. As apparatus become more complex for creating and monitoring effects—think linear accelerator or neutrino detector—the quantity of "built-in" generalizations, especially esoteric theories, expands greatly, perhaps exponentially. Users of these devices, such as particle physicists, seldom know every built-in theory but act as if the devices enable "observation" of the unobservable.

The predictive capability of an observation statement becomes evident when it is communicated to others. Let us assume that a mountain climber, altimeter in hand, is

[1] My use of the term "register" here differs from its use in my cognitive model of human responses (Schiffer and Miller 1999).

the first person to climb a certain peak; he or she reports in a blog that the mountain's height is 6,969 m. After people acquire that knowledge from his or her blog, they would expect to read the same height on their altimeters when climbing the same peak. Now imagine two technicians monitoring an experiment in a laboratory of pneumatic chemistry, with one about to take over for the other. The first technician reports the experiment's progress to the second one by calling out the temperature and pressure readings. Thus, the second technician arrives at the apparatus with specific expectations about what he will see on the gauges. A farmer returns to the village with news that his or her field of young maize has been ravaged by deer. Armed with this knowledge and headed to the field, his or her kin would expect to find many munched plants. In every society observations communicated to others prompt expectations that in turn may influence the conduct of activities.

Although enabling limited predictions, many observation statements are a highly transient form of scientific knowledge, situated at a specific place and time and not always repeatable because performances change. Although a mountain's height is stable over the short term, it may rise or fall over millennia in response to volcanism, glaciation, or the movement of tectonic plates. An apparatus' temperature and pressure may vary rapidly over a short period. A maize field may avoid the predations of deer the following year. Some observations, however, are relatively stable over long time spans: the diameter of Earth at the equator is 12,756 km, a value apt to change little over the millennia.

As noted above, the apparatus for making a specific observation may undergo changes. This typically happens when new resources become available, such as materials and generalizations, which the investigator exploits to design a new instrument. Many interesting research projects could strive to explain changes in the apparatus used to observe a particular phenomenon (on the history of scientific observation, see Lunbeck 2011).

Categories and Classifications

A category is the term applied to a class of interactors such as a *kind* of vegetable or *type* of screwdriver. The defining attributes of class membership—i.e., its referents—depend on sensory performances. Thus, a chair is a kind of thing whose visual performances include those of four legs, a back, and a seat (which are themselves categories). Application of the term chair indicates that the thing so labeled exhibits the necessary visual performances. Classifications are formed from sets of related categories such as furniture, mammals, or planets. Together, categories and classifications make possible the activity of identification: determining whether a particular term can be properly applied to a given interactor. Established categories are learned by people socialized in a community of practice (Barnes, Bloor, and Henry 1996:26). Because the vast majority of categories and classifications are learned, they may be modified, deleted, or augmented in light of new experience or new theory.

Categories do not exist in an epistemological vacuum, for each class is bundled with properties and performance characteristics that give it predictive capabilities (cf. D'Andrade 1995:115–120; Rothbart 2007:9). Anthropologists learned long ago that the terms in a kinship classification describe much more than biological or social relationships. A kin term, in fact, denotes a social role that entails performance expectations: rights and duties (Radcliffe-Brown 1924). "Grandmother" in America labels a kin relation and also a social role that includes expectations about how that person will interact with others, such as being highly indulgent toward grandchildren. Thus, we see that categories and classifications, by virtue of associated generalizations, make possible predictions.

A contrived example illustrates just how expansive these predictive capabilities can be. In a traditional North American society, the category corresponding to "cottontail rabbit" labels a smallish, fur-bearing creature having four legs, large and usually erect ears, and a stubby white tail, all of which perform visually and permit the animal to be identified. Many generalizations about nature, people, and artifacts attach to this category, such as the rabbit's preferred habitat, the best season and time of day to hunt it, appropriate hunting equipment, the size and age-gender composition of the hunting party, artifacts needed for butchering and cooking, and uses for its fur and bones. In short, the simple category "cottontail rabbit" includes *society- or group-specific* generalizations responsive to the roles these critters play in the society or community of practice.

With the advent of "ethnoscience" during the 1960s, cultural anthropologists studied traditional categories and classifications beyond kinship, everything from plants to diseases. Although ethnoscience withered somewhat under the misplaced criticism that it failed to provide direct access to a native's psyche, interest in building models of knowledge survives as cognitive anthropology. "Cognitive anthropology investigates cultural knowledge, knowledge which is embedded in words, in stories, and artifacts, and which is learned from and shared with other humans" (D'Andrade 1995, xiv; see also Atran and Medin 2010; Hutchins 1995).

The contributions of ethnoscience and cognitive anthropology—properly regarded as models of knowledge not as proxies for "psychological reality"—provide additional evidence that a category implicates generalizations that permit predictions. In ethnographic research among native Tzeltal speakers in Chiapas, Mexico, Metzger and Williams (1966) obtained a list of categories roughly subsumed by the English term "firewood" (p. 390). Their study showed that each category—a kind of tree—has "correlates in behavior;" that is, performance characteristics enabling the anticipation of how its wood would fare during use (p. 395). Thus, six kinds of trees are said to yield "good" firewood, each of which is correlated with the following performance characteristics: "hard wood," for example, "burns strongly," "dries rapidly," and "its fire is hot" (p. 397). There is also a list of poor kinds of firewood and their corresponding performance characteristics.

The Tzeltal firewood categories and bundled performance characteristics allow predictions: when one kind of wood is collected, it may be expected to burn strongly (good), whereas another kind burns quickly (poor). Firewood categories are included in a more general set of categories relating to wood use (e.g., roof construction),

which themselves are included in even more general categories (e.g., natural things). Together, these hierarchically related categories make up a classification or classificatory system.

The modern physical sciences also have categories and classifications that, through associated generalizations, permit predictions. The periodic table of chemical elements is a classificatory system developed in the nineteenth century by Dmitri Mendeleev. He used the table to predict properties of elements as yet unreported; subsequent discoveries of germanium, technetium, scandium, and gallium confirmed his predictions (Scerri 2007). Each element, a type of atom, has many associated properties. One of the first tasks of an investigator claiming to have discovered a new element is to characterize its basic properties (which are "experimental laws," see below). Today, chemical handbooks list for each element dozens of properties, such as copper's color, melting point, tensile strength, coefficient of thermal expansion, specific gravity, and the kinds of bonds it forms with other elements. By consulting the generalizations in a handbook, investigators are able to predict which elements are most appropriate for their projects.

A classificatory system may be theoretically inflected or determined. Thus, cladistics is a form of biological classification that helps to clarify clades, which consist of a founding species and all of the species descended from it. Assigning species to a clade depends on the presumed relevance of traits for indicating genetic relationships. Other biological classifications rest on different theoretical assumptions and different referents, such as cold-adapted mammals, migratory birds, and luminescent marine invertebrates.

A common trope in discussions of science is the dismissal of categories and classifications as "just description," implying that they are inferior forms of knowledge, perhaps typifying the earliest stage of science—or they are not science at all, more akin to "butterfly collecting." These views overlook the central role of categories and classifications in human life: enabling predictions essential for the forward motion of *all* activities and for conducting generalization-seeking investigations. And, as noted above, generalizations of every kind necessarily incorporate categories.

Generalizations

Empirical Generalizations

An empirical generalization describes the repeatable performances exhibited by members of a class of interactors *whose occurrence is confined to a specific time or place or society*. In contrast to an observation (as defined above), an empirical generalization qualifies as a *generalization* because it applies to a group of interactors rather than a singular one. Especially important are plant and animal categories, each of which has a finite duration in time and often a restricted distribution in space. Thus, the *category-specific* properties and performance characteristics of plants and

animals are empirical generalizations that permit predictions, and thus make possible subsistence and many other activities.

In archaeology, every culture-historical artifact type is bundled with empirical generalizations that implicate time–space referents. Thus, archaeologists in the American Southwest have shown, by means of associated tree-ring dates, that the pottery type known as "Four Mile Polychrome" was made in east-central Arizona during the fourteenth century C.E. (Carlson 1970). Possessing this knowledge, an archaeologist could predict that, in visiting a late prehistoric site in that area, he or she would find sherds of Four Mile Polychrome.

Some empirical generalizations correspond to what might be called "empirical reality," whereas others do not. Healing activities in traditional societies, even in recent industrial societies, are infused with empirical generalizations of variable validity. Validity aside, empirical generalizations about the human body's ailments and appropriate remedies affect predictions and healing activities. Suffering a severely infected throat, George Washington was treated by doctors who forecast that bleeding, emetics and purgatives, gargling with sage and vinegar, and application of a wheat-bran poultice might promote recovery. Instead, America's retired first president died, perhaps killed inadvertently by his doctors who relieved him of 5 pints of blood on the basis of erroneous empirical generalizations (Schiffer, Hollenback, and Bell 2003:133–134).

Experimental Laws

Along with empirical generalizations, experimental laws are the quanta of *relatively* stable empirical knowledge whose predictions contribute to the forward motion of activities. Much like empirical generalizations, experimental laws have substantial empirical content, *but lack temporal, spatial, and societal referents*. Rather, experimental laws are defined on the basis of the performance or performances of one or more interactors. Another way to express this definition is that experimental laws describe the operation of a specific process—a set of closely related interactions or activities. Because processes work in patterned ways, performances may be described by deterministic, statistical, or probabilistic expressions. But, as Cartwright (1994) emphasizes, experimental laws are always qualified by parameters or (boundary) conditions. And, like empirical generalizations, experimental laws vary in validity.

Let us take a very simple example: the process of boiling water. The experimental law is that water boils at 100 °C, but it does so only under the boundary conditions of standard temperature and pressure (0 °C, and 1 atm). Rendered behaviorally, this law describes a process in which a capable heat source performs thermally to raise the temperature of water until, at 100 °C, the water exhibits the distinctive visual and acoustic performances known as boiling. I propose that virtually any experimental law may be put into behavioral terms (cf. Reichenbach 1966:16).

Experimental laws also describe behavioral processes in artifact life histories, including manufacture, use, reuse, and deposition, as well as environmental

processes that affect artifacts, features and structures, and regions (Schiffer 1976, 1996, 2010). The basic processes for flaking stone (e.g., hard-hammer, soft-hammer, and indirect percussion; pressure flaking) are described by process-specific laws. Hard-hammer percussion, for example, requires three interactors: hammerstone, core, and knapper, each of which has the properties and performance characteristics necessary for that process. The hammer is a hard, tough material, either stone or metal; the core is a brittle material, such as glass or cryptocrystalline quartz, that has a suitable striking platform—an edge with a platform angle of about 90°C or less. The knapper is capable of delivering a deft and energetic blow to the striking platform while firmly grasping the core in one hand and the hammerstone in the other. The predicted performance—the removal of a flake from the core—is a probabilistic outcome of that interaction and depends on a host of factors.

People in traditional societies deployed, implicitly and explicitly, myriad experimental laws pertaining to processes of artifact manufacture. Passed down in technological traditions within communities of practice, these laws allowed people to forecast that undertaking particular interactions with tools and materials would lead, often enough, to the anticipated outcome. Of course the artisan also had to develop the necessary skills.

Since Semenov's (1964) foundational work on the alteration of chipped-stone tools through use processes, archaeologists' experiments on chipped stone (e.g., Keeley 1980), bone (e.g., Griffitts 2006), pottery (e.g., Skibo 1992, 2013), ground stone (Adams 2002), and other tool materials have vastly increased our corpus of experimental laws of use alteration. Tool users in the past employed some of these laws. When an artifact was observed to perform ineptly owing to accumulated wear, such as a chipped-stone knife too dull for cutting, the user might have conducted maintenance activities (retouch).

Experimental laws, in contrast to empirical generalizations, are susceptible—eventually it is believed—to theoretical explanation (Nagel 1961). For example, the process of hard-hammer flaking can be explained by mechanical theories of brittle fracture that involve hertzian cones and the propagation of the applied force as a wave within the core—an inelastic solid (Cotterell and Kamminga 1990). However, experimental laws are often formulated long before investigators propose theories to explain them. In traditional societies, the experimental laws that manufacture processes embody may have elicited little theory-building; in industrial societies, the theoretical explanation of experimental laws occupies much effort.

Significantly, valid experimental laws are highly stable generalizations and tend to survive the demise of theories that explain them (Nagel 1961:87). Hall (1956:34–35) has noted that "higher-order generalizations are vulnerable, but in descending the scale to the substratum of experimental fact the chances of serious error steadily diminish." Over the long term, theories come and go but valid experimental laws change little (cf. Kuhn 1970). In any event, we cannot escape the conclusion that different theories following each other in time may explain the same empirical process(es). Moreover, an anthropological perspective also suggests that, at any given time in the same society, different theories may function as knowledge in different communities of practice. Thus, to explain hard-hammer flaking, archaeologists employ a behavioral explanation, whereas engineers employ brittle-fracture theory.

Recipes

Empirical generalizations and experimental laws are the two kinds of scientific knowledge that allow people to anticipate the consequences of *specific* performances and interactions, and so enable simple and immediate engagements with other interactors. But many activities—and especially complex processes—involve a higher level of organization and a more extended engagement with the material world than empirical generalizations and experimental laws, *taken individually*, can describe. The need for a more appropriate kind of knowledge is satisfied by *recipes*. Krause (1985:29–31) first called attention to the importance of recipes for modeling manufacture processes (see also Schiffer and Skibo 1987), but recipes also apply to other complex activity sequences and render their outcomes somewhat predictable.

A complete recipe consists of several elements (adapted from Schiffer and Skibo 1987). (1) A list of the quantities and *relevant* properties and performance characteristics of all interactors (people, artifacts, environmental phenomena). (2) A detailed specification of the interactions, organized in sequence(s), including alternative sequences that are equally effective or that may be needed in response to changed circumstances. (3) A statement of the expected outcome. A successful recipe describes sequences of interactions and activities that, when carried out competently, are likely to yield an outcome such as an artifact's procurement, manufacture, maintenance, reuse, or cultural deposition. Partial recipes reconstructed on the basis of archaeological evidence may nonetheless capture important details of past science and technological practice. I stress that all explicit recipes, ancient and modern, are *models* created by a practitioner, observer, or the archaeologist.

Let us focus on manufacture recipes, which are familiar to us in cookbooks, other how-to books, and in many archaeological reconstructions and older ethnographies. Models of manufacture recipes have great antiquity: clay tablets excavated in Mesopotamia include instructions for making perfumes, dyes, and soaps (Levey 1959). The perfume recipes, which documented women's science and technology, were written in Akkadian and dated to 1256–1209 B.C.E. A corpus of recipes recovered from King Assurbanipol's library at Ninevah (seventh century B.C.E.) tells how to employ common materials to make glass of various colors that mimic natural stones such as lapis lazuli (Oppenheim et al. 1970).

When a manufacture recipe is correctly followed, the expected outcome may range from colored glass to catfish stew to a nuclear-powered submarine. The overall interaction sequence may be of any length and often specifies intermediate and alternative outcomes, the latter depending on common contingencies that the artisan(s) may encounter. Some recipes admit flexibility in the order of interactions: when making guacamole from scratch, it matters not whether tomatoes or onions are chopped first. The sequence(s) may also include interactions occurring simultaneously in activities at different places, which is common in the recipes of industrial processes. The abstract structure of the most complex recipes may be compared to a lengthy computer program having many subroutines.

Once established in traditional societies, recipes are communicated by interpersonal teaching and learning strategies, and maintained by the practitioners of technological traditions. Modern industrial societies have available many more modes of knowledge transmission, from apprenticeship to computer simulations, most of which involve artifacts. Recipes are materialized today in nearly every print and electronic medium.

Manufacture recipes in all societies may include rituals because of the belief that such performances are needed. People thus carry out the interactions, confident that the ritual will help to create the product (Malinowski 1954). Thus, some Sumerian glass recipes called for sacrificing a sheep and burning incense (e.g., Oppenheim et al. 1970:44). Together, technical performances based on scientific knowledge and rituals buttressed by beliefs in "non-immediate sources of power, authority, and value" (Bell 1997, xi) together yield tangible outcomes—e.g., the smelting of iron in an African village (Childs and Killick 1993) or the construction of a canoe for deep-sea voyages in Melanesia (Malinowski 1961). The "non-immediate sources of power, authority, and value" may be propitiated, placated, appealed to, avoided, obeyed, resisted, or merely referenced in rituals that alternate with or accompany technical performances.

Our models of recipes tend to lack rituals, which may leave scant archaeological traces. By drawing on ethnographic evidence and generalizations, however, we may be able to infer rituals that probably took place. Following Malinowski (1954), for example, we should expect rituals to occur when specific interactions are risky to participants, very difficult to perform, or have uncertain outcomes. The Sumerian glass-making rituals predictably occurred prior to the failure-prone final melt (cf. Fischer 2008).

Why do recipes usually yield the expected product? The answer is that beneath the sequence of visible interactions, activities, and intermediate outcomes lies an invisible realm of empirical generalizations and experimental laws. Thus, a recipe's interactions are in accord with, indeed depend upon, the validity of other nuggets of scientific knowledge. Asking how any recipe works its magic, then, leads directly to the exploration of this hidden realm, to research projects that bring to light the implicit generalizations. It is precisely these underlying generalizations that make it possible for a recipe, when followed skillfully, to create something entirely new to human experience—and to the universe.

People in every society invent recipes by acquiring the relevant generalizations, usually through trial and error. Let us envision a person developing a clay cooking pot for use over an open fire. His or her first attempts are likely to fail, but each failure leads to a change in technical choices, and eventually the aspiring potter may devise a successful recipe, perhaps one incorporating rituals. We may approximate this learning process by following the pot's behavioral chain, positing the necessary generalizations, as in the following (see Schiffer and Skibo 1997):

1. If the potter chooses a local raw material containing enough clay to be workable, then a vessel can be formed.
2. If the potter in creating the paste adds enough nonclay particles to the clay, such as animal dung or sand, then the vessel is likely to dry without cracking.

3. If the vessel's paste contains ample nonclay particles and the potter dries the vessel thoroughly, then it is likely to survive firing.[2]
4. If the potter fires the vessel at a sufficiently high temperature, then it will have adequate strength and maintain its integrity during use.
5. If the potter treats the vessel's interior surface to make it somewhat impermeable, then it will heat its liquid contents effectively.
6. If the potter makes a globular vessel with walls of even thickness, and has added enough nonclay particles to the paste, then it will survive repeated heating/cooling episodes during use.

The appropriate raw clay and nonclay particles, once identified, can be specified as empirical generalizations. In many societies firing sometimes fails to produce the expected result, and so firing is often preceded by a ritual. As the recipe is developed and put into practice, the potter acquires the necessary skills.

Once the making of cooking pots became routinized as recipes perpetuated in a technological tradition, there was no need for experimental laws and empirical generalizations to be explicit. They were likely to "surface only during times of experimentation (if at all)" (Schiffer and Skibo 1987:597). Indeed, practitioners in traditional societies seldom supply generalizations when answering questions about why they engage in certain interactions. Often the response is simply, "that's the way we've always done it." A conversation-stopper, this answer invites the archaeologist to model the underlying generalizations. The modeling process requires a sophisticated understanding of the technology, as gained through experiment (see the chapter "Contributions of Experimental Archaeology"), inference from behavioral observations, archaeometric studies (see "Contributions of Archaeometry"), experience as a practitioner, or studying modern scientific and engineering texts. Even in industrial societies modeling is often necessary because practitioners—from bakers to air conditioner technicians—merely follow long-standing recipes. Modeling a recipe may suffice for some archaeological projects, but sometimes our research interests will also lead us to uncover the implicit generalizations.

The pottery example supports the claim that a recipe's interactions are underlain by generalizations, which render their consequences somewhat probable. It follows that there is no rigid boundary between the content of a complex experimental law and that of a *very simple* recipe. As noted above, several experimental laws describe hard-hammer percussion, but that process also conforms to a recipe in which those experimental laws are implicit. I emphasize, however, that *recipes are essential for modeling longer and more complex interaction and activity sequences*.

Different recipes may result in a similar product (an instance of equifinality). Thus, by following any of several recipes, a potter may form vessels having similar formal properties and performance characteristics. And, in the replications of Folsom points, experiments have shown that many techniques can produce the characteristic channel flakes (see chapter "Contributions of Experimental Archaeology").

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[2]There are exceptions: some natural clays already contain sufficient nonclay particles to permit successful drying, firing, and repeated use over a fire.

Archaeologists and historians have long known that new technologies have sometimes given rise to new science (e.g., Staudenmaier 1985). But my claim is stronger: new scientific knowledge—e.g., the recipe for how to make or use something—is a necessary consequence of all technological development (see "Science: A Behavioral Perspective").

Discussion

Some readers may be surprised that I include recipes as a kind of scientific generalization. After all, recipes for making a cooking pot or pumpkin pie connote mundane activities accessible to almost anyone. Yet, Robert Boyle, chief exponent of experimental methods in early modern science, "sought to acquaint himself with the practical procedures employed by tradesmen and artisans in their manipulations of nature" (Sargent 1994:67; see also Hall 1956:218–222, 308–309). Boyle's familiarity with the activities of ordinary people—instead of immersion in tracts authored by ancient philosophers—was salutary because, he argued, "the 'phenomena afforded by trades' must be made a 'part of the history of nature,' because they may 'both challenge the naturalist's curiosity, and add to his knowledge'" (Boyle, quoted in Sargent 1994:67). From tradesmen and artisans, Boyle learned recipes for making things as well as the properties of materials—the kinds of knowledge that were integral to his natural philosophy.

In addition to Boyle's historical precedent, there are other grounds for arguing that recipes are a kind of scientific generalization. First, we may model recipes as a complex expression compounded of both empirical generalizations and experimental laws. Let us represent the simplest possible recipe as "if *X*, then *Y*," where any *X* is an interaction among specific ingredients and tools, and *Y* is that interaction's product. By itself, this statement is equivalent to an empirical generalization or experimental law. By expanding this expression, we may represent the steps of any recipe of greater complexity. To wit, if X_1, then Y_1; if X_2, then Y_2;…. if X_n, then Y_n (inspired by a discussion in Gooding 1990b:113). Thus, a recipe may be considered the shorthand expression of a compound and very complex generalization.

Second, recipes—and recipes alone—permit people to create, *and researchers to explain*, an emergent empirical phenomenon (the outcome). An important implication is that, by knowing only ingredients, tools, and relevant generalizations, we would be unable to anticipate or explain the outcome, for the latter is determined by a recipe's *interaction sequences*.

And third, the main function of scientific knowledge, as defined behaviorally, is that it empowers people to engage competently with the material world. Recipes meet this criterion especially well because they make possible the interaction sequences of behavioral chains, from processes of material procurement to cultural deposition.

Theories

The theories of modern science are arguably its most significant and perhaps most versatile achievement, for they are thinking tools that promote, often through predictions, apparatus-intensive investigations and discoveries. Because theories also provide the highest level of explanation of which humans are capable, some empirical phenomena are explained theoretically in every society. Our big brains with seemingly limitless storage capacity, extraordinary executive functions, and symbolic language give us the ability to posit underlying causes, be they spirits or atoms.

Theories can be defined as follows (simplified from Nagel 1961). A theory is an abstract and often complex expression, having little if any empirical content but possessing empirical implications. Theories explain patterns in phenomena by invoking causal agents—entities and processes—that cannot be observed by unmediated human senses. This definition requires unpacking:

1. "Abstract expression." The core of a very simple theory may be one abstract statement such as "all gases consist of molecules." Commonly, however, theories such as plate tectonics and natural selection consist of many interrelated abstractions.
2. "Having little if any empirical content but possessing empirical implications." Einstein's theory of general relativity attributed gravitation to the ability of matter to warp space and time, a decidedly nonempirical claim. However, this counter-intuitive theory explained Newtonian anomalies, including the precession in the perihelion of Mercury's orbit, and yielded verifiable predictions such as the gravitational bending of light.
3. "Explain patterns in phenomena by invoking causal agents—entities and processes—that cannot be observed by unmediated human senses." Entities often defy direct observation because they are too small, too large, too distant, or too abstract, and processes may occur on a time frame too short or too long to be observed without apparatus. Theoretical agents include quarks, black holes, and tree spirits; and processes include photosynthesis, radioactive decay, and mitosis. As the previous sentence implies, theories invoke a remarkable variety of unobservables. With symbolic language we refer to entities and processes in the past, present, and future—even distant in space. Not only does the boundless productivity of language make possible theory-building, but it also promotes the inter- and intra-generational communication of scientific knowledge.

As many students of modern science have pointed out, theories are often ill-defined generalizations (e.g., Barnes, Bloor, and Henry 1996:94, 105). This means that predictions and theoretical interpretations of phenomena may be loose and fluid, enabling investigators to modify a theory's meaning so as to seek or accommodate new observations (and generalizations). In atomic theory, representations of the "atom" have been revised many times during the past two centuries in response to (1) new experimental evidence about its makeup, (2) the performance of elements in chemical reactions, and (3) the implications of other theories such as quantum chromodynamics. Consequently, the atoms envisioned by Dalton, Mendeleev, Rutherford, Bohr, and Gell-Mann differ

greatly, ranging from solid spheres to planetary-like systems to combinations of wave functions, quarks, and gluons. And each representation implied different predictions and led to different experiments.

Entities and Processes: Natural, Quasi-Natural, and Supernatural

Beginning in the seventeenth century, practitioners of early modern science sought to set off their theories from others by insisting that entities and processes must be natural not supernatural. "Natural" is ordinarily understood to be material in contrast to something in the spiritual realm whose existence is maintained *exclusively* through faith. The dichotomy of natural and supernatural entities seems clearcut, but is not. When a scientific theory is first advanced, its unobservables may not appear to be material at all. René Descartes, though an ardent advocate of material processes, created models of invisible vortices that purportedly accounted for planetary motion, light, and magnetism (Hall 1956). In explaining why the speed of planets in their orbits decreased with distance from the sun, Kepler invoked a "moving spirit" (*anima motrix*) that inhabited the sun whose force weakened over longer distances (Hall 1956:123). In his celestial mechanics, Newton rejected Kepler's moving spirit yet relied on an unobservable force—gravitation—that had material effects but no apparent material existence. According to Newton, "All bodies … are endowed with a principle of mutual gravitation" (quoted in Shapin 1996:61). Because gravitation itself could not be represented as a material phenomenon, Leibniz viewed it as occult (Shapin 1996:42, 63).

Modern investigators have been no less prolific in positing materially ambiguous unobservables. Some of them—e.g., antimatter, black holes, and gene—gained acceptance as material phenomena when supported by experiments with new apparatus and buttressed by further theoretical developments. Others were abandoned or, like dark matter, strings, and sterile neutrinos, remain in the realm of the "possible." To have believed at first that all such unobservables were material phenomena was partly an act of faith. Recently, theoretical physicist Patrick Huber, referring to the difficulties of validating "sterile neutrinos," remarked in *Science* that "It's like trying to prove the existence of God" (Huber, quoted in Cho 2011). Let us apply the term *quasi*-natural to theoretical agents that are not unambiguously natural or supernatural, acknowledging that they may later become natural, fall into disuse, or be deemed occult or supernatural.

Tree spirits and gods fall beyond the scope of modern science, not merely because they are clearly supernatural, but because their ascribed dispositions produce erratic—i.e., unpatterned—performances. Whether occult or not, gravitation yields experimental laws having precise predictions. A malevolent tree spirit may cause misfortune, a god may unleash a furious storm, but not even probabilistic laws could ever link these entities to their purported effects. Even so, people attempt to control the uncontrollable through rituals: giving offerings to a tree spirit, praying to a god. Such a ritual's manifest function is unachievable, but its performance often has beneficial social and psychological effects on participants and witnesses.

Like gravitation, quasi-natural entities and processes may have patterned effects and so can be investigated. In search of predicted effects, investigators develop mediating apparatus. Throughout the twentieth century, for example, a succession of complex devices has been built in search of theorized subatomic particles, and many were "observed." Proposals have been put forward to build apparatus that might detect sterile neutrinos, but funding is uncertain (Cho 2011). Archaeologists of science could profitably study the succession of apparatus constructed to yield evidence of a quasi-natural entity's existence. It might be interesting to focus on a quest that ended in failure.

Models

Models, which furnish potentially useful simplifications of reality, may be symbolic or mechanical (in two or three dimensions). We are all familiar with mechanical models of molecular structure, but at the cutting edge of science today they have been largely superseded by computer models, some of which simulate the folding of proteins in living cells and express the results in polychrome graphics. Dynamic models iterate complex processes over long periods such as Earth's climate system, nuclear fusion, and the birth of the universe. Digital models are highly malleable, as investigators tinker, adding and subtracting parameters and variables, and altering the latter's values.

Models are not new. William Gilbert (1958[1600]), court physician to Queen Elizabeth, wrote about his model of Earth, which he called a *terrella*. It was a globe of magnetite, which is a magnetic mineral. In experiments with his *terrella*, Gilbert showed that Earth itself could very well be a magnet. Beginning in late medieval times in the West, there was a proliferation of highly visible machines that affected peoples' lives, including clocks, looms, and windmills. Significantly, these technologies also provided models for thinking about nature. Yet, as Shapin (1996:30) notes, "the very idea of construing nature as a machine, and using understandings derived from machines to interpret the physical structure of nature, counted as a violation of one of the most basic distinctions of Aristotelian philosophy … the contrast between what was natural and what was contrived or artificial." Despite controversies over their use, mechanical models of natural processes continued to be employed. By the end of the seventeenth century, the clockwork model of the universe, handiwork of humans, had prevailed, while the universe itself presumably had been constructed and set in motion by a deity.

Archaeologists can study surviving mechanical models just as they study any artifact, with perhaps the added availability of oral history and documentary materials. As for digital models, I do not foresee archaeologists rooting around in software, but other kinds of research seem possible, such as learning how changes in computer hardware affected the kinds of systems that could be modeled as well as the complexity of models.

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Part II

Contributions of Experimental Archaeology

Experiments yield knowledge about a wide range of subjects that contribute to archaeological recovery, analysis, and inference (for recent overviews, see Coles 1979; Cunningham, Heeb, and Paardekooper 2008; Ferguson 2010; Mathieu 2002; Millson 2011; Saraydar 2008; Shimada 2005; Skibo 1992, chapter 2). Archaeologists also do experiments to illuminate the science of prehistoric societies.

This chapter presents examples and case studies that illustrate two experimental approaches for modeling a technology's scientific generalizations: (1) replication or imitative experiments (Ascher 1960), which produce recipes, empirical generalizations, and experimental laws, and (2) controlled experiments that yield experimental laws (Schiffer et al. 1994).

Replication Experiments: General Considerations

Archaeologists have encountered many artifacts—from copper spearheads to minuscule beads—whose manufacture processes were at first unknown, perhaps somewhat mysterious, especially if the technology had no modern practitioners. To fill these inferential gaps, researchers may replicate the manufacture process, as in reverse engineering. In addition to the finished product, archaeologists may have available other lines of evidence such as waste products, by-products, tools used in manufacture, and manufacturing locations.

The path to a replication experiment usually begins when the archaeologist asks a behavioral question of a puzzling artifact: By which manufacture processes was it made? Archaeologists might also ask how a particular artifact was used or reused. Any behavioral question can provoke a replication experiment, but I focus here on manufacture processes.

In a replication study, we seek authenticity, striving to recreate the exact sequence of interactions and activities, using the same materials and tools as past people (Flenniken 1981:2). A replication may fall short of this gold standard, but the

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research may still produce a valuable partial or skeletal recipe (e.g., Frison and Bradley 1980:51–52). Let us now turn to several idealized steps for modeling a manufacture recipe.

1. Infer the raw materials. For ceramic and metal artifacts, this information is supplied by petrography, neutron activation analysis, electron microprobe, or other archaeometric tools (see chapter "Contributions of Archaeometry"). Zooarchaeologists can often identify the element as well as the genus or species from which a bone artifact was made.
2. Learn where in the environment the raw materials occur(red). The sources of ground and chipped stone are revealed by a geological survey as are sources of metal ores, clay, temper, and pigments, augmented by archaeometric analyses. The distribution and prevalence of modern fauna hint at which species might have been available, at least for historic times and later prehistory. In well-known regions, information about raw material sources may already be in the literature. Visits to source locations provide samples for experiments, and produce empirical generalizations.
3. Infer basic manufacture activities by seeking distinctive traces, perhaps through archaeometric analyses (e.g., Malainey 2011). Thus, microscopic inspection of a polished metal section may supply information about annealing, cold-hammering, amalgamating, and other treatments. For pottery, petrographic analysis, dilatometry, refiring, and differential thermal analysis are used to estimate the original firing temperature, which has implications for how firing was carried out.
4. Infer the tools that might have been used in the manufacture process by drawing on the following lines of evidence, when available: (1) traces of manufacture on the artifact itself, (2) tools found in the same site assemblage or in the region, (3) tools associated with the artifact in special contexts such as a burial, cache, or workplace, and (4) waste products and by-products of manufacture. Behavioral chain analysis sometimes provides inferences about which tools performed specific interactions, even if they are absent from the immediate archaeological record (Schiffer 1975). Analysis of use-alteration traces on suspected tools may furnish supportive evidence.
5. Draft a recipe consisting of a sequence of interactions among artisan, tools, and raw materials.
6. Follow the recipe and assess the outcome.

A successful replication meets the following conditions: (1) relevant attributes of the replicated artifact are essentially identical to those of the original specimens, (2) waste products and by-products of manufacture match those from the archaeological record, and (3) use-alteration traces on the tool(s) match the archaeologists' expectations. In practice, the first and second conditions are often taken to be definitive.

Early trials often fail, and so replication is usually an iterative process. A failure leads to tinkering with the recipe, as in substituting different materials, tools, or interactions. Often, however, the problem stems from insufficient practice, as many manufacture processes have a rather steep and lengthy learning curve. It may require

several years of part-time practice to become a proficient potter or flint knapper, for skill is acquired only through repetition and the fine-tuning of interactions.

As successive iterations begin to show consistency in meeting the conditions for success, we may claim to have created an accurate model of the recipe. We are well aware, however, that a claim may falter in the face of equifinality because it is possible—in principle—for different tools and interactions to replicate a given artifact (Ascher 1960). In practice, a recipe is likely to stand until challenged by an equally successful alternative. To distinguish between recipes, we may examine new attributes of the artifact, waste products, and tools; even then, arriving at a definitive recipe may be impossible. Nonetheless, differences in competing recipes may be judged trivial by archaeologists not invested in the replications.

Many recipes of prehistory are incomplete because we seldom find evidence of ritual activities that might have occurred between or alongside technical interactions (see chapter "Varieties of Scientific Knowledge"). One hypothesis furnishes some guidance: when the outcome of an interaction or activity is uncertain (a probability somewhat less than 1.0), the artisan may have performed a ritual to ensure a successful outcome. Firing pots and smelting metals are failure-prone activities that may be highly ritualized. If ethnographic or ethnohistoric accounts of manufacture-related rituals are available, the archaeologist may hypothesize their occurrence and seek any subtle traces.

Experimental Laws and the Modern Era of Flintknapping Experiments

While conducting a replication study, the archaeologist learns the *consequences* of particular interactions—i.e., rediscovers the underlying generalizations. A simple example comes from heat treatment of chipped stone. Early ethnographies that mentioned the heating of stone prior to chipping were once dismissed as fanciful because no modern knappers used this process and because archaeologists themselves were unfamiliar with it. During the 1960s and 1970s, Don Crabtree and others tried different heating regimes and assessed their effects on flakeability (e.g., Crabtree and Robert Butler 1964). They found that heat treatment improves the flakeability of certain materials by increasing its brittleness. Further experiments showed that heat treatment also affects a stone's color and luster. By seeking the latter traces in prehistoric assemblages, archaeologists have shown that heat treatment was practiced by many an ancient knapper. Built into their technologies, then, was an experimental law: heating certain kinds of stone using a specific regimen (low heat, perhaps beneath a camp fire, applied for many hours) makes the material easier to chip into projectile points, knives, and so forth.

The heat-treatment example is typical: the earliest attempts to replicate a specific technology usually resulted in the recognition of causal relationships that may be represented as experimental laws. Thus, William Henry Holmes, who in the late

nineteenth century was among the first researchers in the USA to replicate bifacially chipped stone tools, originated several generalizations that remain valid today. With words and a drawing he described the interactions by which a person can use one cobble to strike a flake from a second cobble: “Grasping a bowlder [i.e., cobble] in either hand (supposing bowlder hammers to have been used), the first movement was to strike the edge of one against that of the other at the proper angle to detach a flake (Fig. 10)” (Holmes 1897:59). Holmes’ drawing shows the relative position of hands and cobbles, and also indicates the “proper angle” of the blow. As noted in chapter “Varieties of Scientific Knowledge,” an empirical generalization or experimental law underlies each interaction in a recipe and makes a specific outcome probable. Because these generalizations are implicit in the technological tradition, they are unlikely to be elicited from an artisan (Schiffer and Skibo 1987).

Although the roots of flintknapping experiments are centuries deep (Johnson 1978), the modern era began in the 1960s on both sides of the Atlantic. The need for experiments arose because the few remaining knappers in traditional societies used only the most rudimentary techniques whose science was inadequate for modeling the recipes of more challenging technologies. In France, prehistorians François Bordes and Jacques Tixier became expert knappers and stimulated widespread interest in replication. In the USA, Don Crabtree, a self-taught knapper, revived interest in chipped-stone technologies and, in summer field schools, passed along his knowledge to dozens of archaeologists (Whittaker 1994). This heightened activity yielded numerous recipes for many artifact types and, importantly, a host of explicit experimental laws. Crabtree, for example, studied the projectile points from Snaketown, a large Hohokam site in southern Arizona. In a series of experiments he easily replicated these artifacts (Crabtree 1973); his publication also furnishes numerous experimental laws, and so is an excellent primer on flint knapping. A more complete compendium is Whittaker (1994).

There are four major flaking modes: hard-hammer percussion, soft-hammer percussion, pressure flaking, and indirect percussion. Each mode produces flakes and debitage (waste products) whose modal properties can be described by statistical generalizations. Hard-hammer percussion, involving a stone hammer (e.g., Holmes’ experiments), detaches large, thick flakes, as in the earliest stage of roughing out a tool. Hard-hammer flakes generally have a prominent bulb of percussion on the ventral (interior) surface. Requiring a thick piece of antler or bone, soft-hammer percussion removes smaller and thinner flakes that usually lack a prominent bulb of percussion. Final shaping and finishing may be done with pressure flaking in which an antler tine, for example, applies pressure to the edge of the piece and removes a tiny, thin flake. In indirect percussion, a punch of bone or antler is held against the piece and struck with a hammerstone. This flaking mode enables the application of much force to a very small spot and allows a larger flake to be pushed off than by pressure flaking.

Once steeped in these generalizations and after a good deal of knapping practice, we can analyze a chipped-stone assemblage and often construct a skeletal recipe of the flaking activities that produced particular artifacts. By following the recipe, we can test these inferences through experiments, filling in details of the interactions.

Folsom Spear Points and the Equifinality Problem

Some artifact types are so difficult to make that replication requires many experiments and results in multiple recipes. This equifinality problem is painfully evident in the scores of attempts to replicate Folsom spear points. Manufactured on the Great Plains of North America, around 10 millennia ago, and used for hunting and butchering bison, these points were made mainly on fine-grained, easily chipped materials such as chert and flint. Folsom points are unusual because on both faces is a wide flute reaching from the base almost to the tip, left by the detachment of a long, thin channel flake (Fig. 1). By all accounts—and there are many—Folsom points are exceedingly hard to make. And to this day replicators have not converged on a just one recipe.

The earliest stages are not problematic: (1) hard-hammer percussion shapes the blank, (2) soft-hammer percussion creates a preform, and (3) careful pressure flaking thins the margins, refines the shape, creates the ears, and forms a nipple at the base that becomes the platform for removing the channel flake (first on one face, then on the other). Although there is some variation in the early stages of recipes, all result in a thin biface ready for fluting. After detailed study of the Lindenmeier Folsom assemblage, Nami (1999) suggested that some bifaces were heat-treated immediately prior to fluting, increasing the likelihood of successfully detaching the channel flake.

The puzzle resides in how the channel flakes were actually detached. A good starting point is the recognition that this task confronted Folsom knappers with

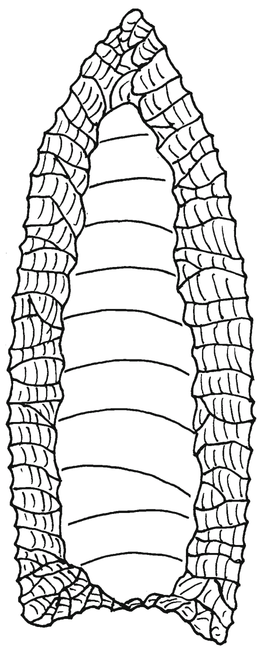

Fig. 1 A highly stylized Folsom point

several problems. The first is that a channel flake has too much mass to be removed by one person using unaided pressure flaking. The second is the need to secure the point firmly but gently while applying considerable force. In solving these and other problems, archaeologists have invented a host of workable techniques, some employing appliances or requiring a second person (see chapters in Clark and Collins 2002; Whittaker 1994:234–242). With a complicated device involving a lever, one person can apply enough force, through pressure, to remove a channel flake from a point held in some sort of vise-like tool. This solution works but was unlikely to have been used by Folsom hunters. A more plausible solution is the use of indirect percussion, perhaps applying force by striking a small bison-bone punch with a hammerstone. When one knapper uses indirect percussion, he needs some sort of holder to secure the point, perhaps fixed between his feet. Two people can also use indirect percussion, but they still need a holder. Many varieties of tools, including holders and supports, have been employed in replicating indirect percussion, but none is accepted as the definitive solution. Further analyses of debitage, bison bones, and other stone artifacts might yield new information for eliminating some alternatives. In any event, experiments have shown that flutes can be flaked in many ways; perhaps Folsom hunters also employed a repertoire of successful techniques.

The Folsom archaeological record holds a plethora of failed attempts to flake the flute. Researchers debate about whether these spoiled specimens were the work of novices or the least skilled knappers; alternatively, perhaps they represent an inherently high risk of failure that even expert knappers could not always overcome. Most likely, all three factors contributed to the failures. If even highly skilled knappers were unsuccessful sometimes, then we might expect that a ritual preceded fluting, perhaps involving bison bones or other bison parts.

Replication experiments have clearly isolated the problems faced by Folsom knappers and demystified the manufacture process by showing how the points could have been made with materials and tools available to ancient hunters. For nonspecialists such as I, who are nonetheless fascinated by these artifacts, the most plausible recipes seem adequate. Indeed, Ingbar and Hofman (1999) suggest that present knowledge of Folsom point manufacture is sufficient; it is time to prioritize other questions about the overarching technological system and its organization.

Controlled Experiments: Surface Treatments and Ceramic Performance

For modeling the experimental laws embodied in many craft traditions, we may turn to controlled experiments. As in replication, a prehistoric (or historic) puzzle may generate the initial interest. However, this interest may lead to the framing of a *general* question, sometimes even to hypotheses, which—unlike replication—are not tied to a particular people, time, or place. To answer general questions, we conduct

experiments that do *not* mimic the activities, tools, and materials of a particular technology, even one that stimulated the research. Rather, the controlled experiment uses materials and tools most relevant for answering the question or testing the hypothesis. Using this approach we can model in the laboratory the experimental laws embodied in *numerous* traditional technologies.

Archaeologists have an abundance of riches when it comes to pottery, for vessels exhibit remarkable variation over time and across space—in size and shape, forming techniques, kinds and quantities of temper, surface treatments, decoration, and firing regime. Moreover, because pottery-making traditions have survived in many places, we can glean many generalizations from the literatures of craft pottery, materials science, ethnography and ethnohistory, and ethnoarchaeology. However, one important topic remained largely unexamined until recently: the experimental laws that underlie the effects on performance characteristics of traditional surface treatments.

Some prehistorians assume that surface treatments, whose attributes help to define culture-historical types, are decorative. In an otherwise illuminating book on pottery technology, Rye (1981:3) stated that "Non-essential operations include burnishing, applying paint or slip, and all other forms of decoration. These are non-essential because they do not affect the serviceability of the product." Other archaeologists hypothesize that some surface treatments might enhance utilitarian functions (e.g., Rice 1987:230–232). During the late 1980s James Skibo and I took up the utilitarian hypothesis and subjected it to a battery of tests in the Laboratory of Traditional Technology at the University of Arizona.

Because of our interest in modeling the experimental laws implicit in many ceramic traditions, we selected surface treatments spanning a wide range of modifications: finger-smoothing, polishing (also called burnishing), slipping and polishing, smudging, organic coatings (varnish in this experiment), and texturing. Next, after considering a variety of performance characteristics pertaining to several utilitarian functions, we tested heating effectiveness, evaporative cooling effectiveness, abrasion resistance, impact strength, and resistance to thermal shock and thermal spalling.

For each performance characteristic we strove to design a behaviorally relevant test—i.e., one that closely simulated likely conditions of use. This chapter discusses only the test of heating effectiveness, which I conducted (Schiffer 1990). Heating effectiveness is the rate at which a pot raises the temperature of its contents, a performance characteristic relevant mainly for some cooking pots. (With the exception of impact strength, which produced no credible results, the remaining tests have been published [e.g., Schiffer et al. 1994].)

For the test of heating effectiveness I made 24 miniature cooking vessels using a commercial clay and added large quantities of sand temper. Each pot had a different combination of surface treatments: six on the interior, four on the exterior. Made in a press mold, the vessels were identical in size, shape, and amount of paste, and were fired simultaneously in an electric kiln.

Some readers might question the use of commercial clay and an electric kiln. But that would miss the point of the controlled experiment, which was to hold constant

every technical choice other than surface treatment. Natural clays exhibit compositional variation, and open firings vary in atmosphere and temperature over short distances. Thus, both technical choices would have introduced uncontrolled variability into the results.

Before testing the pots, I measured the permeability to water of their interior and exterior surfaces, a property that Rice (1987:230–232) suggested could influence a vessel's thermal performance. The measurements, based on the amount of water that a surface absorbed during a given time interval, indicated that surface treatments vary greatly in permeability.

Each pot was filled about two-thirds of the way to the rim with 25 ml of water and covered with a large ceramic tile. In the center of the tile a rubber stopper held a thermometer whose bulb dipped into the water. I then suspended the pot on a ring stand just above a tiny Bunsen burner. Twenty-five seconds after adding the water, I lit the Bunsen burner and clicked on a stopwatch. When the water reached 90°C, I clicked off the stopwatch, which recorded the total heating time.

The variation in heating effectiveness was remarkable. The best-performing pots took about 3 min to reach 90°C, whereas the worst required more than 8 min. Even after 12 min, one vessel never reached the target temperature. The results show that heating effectiveness is markedly influenced by both interior and exterior surface treatments. Vessels with the least permeable interiors (slip, polish, and smudge; and varnish) heated water rapidly; those with the most permeable exteriors (finger smoothed and textured) took longest to reach 90°C. It was easy to offer a hypothesis to explain this pattern. The greater the pot's overall permeability, the faster water penetrates to the exterior surface. Once there it evaporates, removing heat from the vessel's wall at the expense of the pot's contents.

I then reasoned that any vessel with an impermeable interior surface would have good heating effectiveness. To evaluate this hypothesis, I varnished all vessel interiors and retested them. The hypothesis was confirmed: every pot reached 90°C in about 3 min. Clearly, once a vessel's interior surface is impermeable, the exterior surface has no discernible influence on heating effectiveness (see also Young and Stone 1990).

These findings help explain common technical choices—some of them seemingly inscrutable—for making low-fired cooking pots. Interior surfaces are usually quite smooth, sometimes polished and smudged, and many ethnographies report that interiors are treated before their first use with organic coatings such as milk, pine resin, and baby feces (Schiffer 1990). Any of these treatments, the experiment indicates, enhances heating effectiveness.

The first potters in a region who tried making cooking pots learned that a vessel lacking an interior surface treatment took a long time to heat its contents. Depending on fuel availability and cooking practices, this might have been seen as a serious problem. If so, the potter would have tried out several technical choices, perhaps eventually choosing one that reduced the vessel's interior permeability. The satisfactory technical choice, such as polishing or an organic coating, then became part of the technological tradition. In this way an important generalization came to inhere in the cooking pot technology of many traditional societies.

An experimenting artisan may have learned that an impermeable exterior surface also enhances heating effectiveness. But impermeable exteriors are rarely found on traditional cooking vessels. The controlled experiment produced another pattern that furnishes clues as why this is so: pots with an impermeable exterior may develop, on the part of the base in contact with the heat source, a patch of pits or spalls. Only vessels having a low-permeability exterior avoided this fate. These generalizations are easily explained. A permeable interior creates a water-saturated wall; the heat source turns that water into steam, which expands and, having no easy exit through the impermeable exterior, creates pits and spalls on its way out.

Our remaining experiments also supported the hypothesis that surface treatments affect a variety of performance characteristics related to utilitarian functions. Together, the reconstructed science and contextual factors help us understand many technical choices made by clay artisans in traditional societies.

Controlled experiments do not begin and end with the effects of technical choices and performance characteristics on utilitarian functions. We may also ask general questions about how technical choices affect performance characteristics relevant to any activity along an artifact's behavioral chain. Our earlier experiments (Skibo, Schiffer, and Reid 1989), for example, augmented the corpus of generalizations about temper choice, which affects manufacture-relevant performance characteristics such as workability and paste-drying rate in forming, thermal shock resistance in firing, and portability in transport and use. The ability to tease out the science embodied in past technologies is limited only by our creativity in asking general questions and designing controlled experiments to answer them.

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Contributions of Ethnoarchaeology

Another productive research strategy for studying science is ethnoarchaeology, the acquisition of evidence about ongoing activities in *any* society. Ethnoarchaeological fieldwork has a long history, reaching back to the late nineteenth century in the USA (Schiffer 2009), but its practice was sporadic until the 1970s, after which studies and research traditions burgeoned (see David and Kramer 2001). The usual rationale for doing ethnoarchaeology is that it produces information potentially useful for making and supporting archaeological inferences (e.g., Schiffer 1978). Many ethnoarchaeological projects also furnish data that can be used to model scientific generalizations.

Ethnoarchaeology is customarily conducted through participant observation, but researchers also ferret out behavioral data from ethnohistorical and ethnographic reports as well as oral history. Most ethnoarchaeologists work in traditional societies, but opportunities abound for research in industrial societies. *The basic requirement for an ethnoarchaeology project, then, is the ability to draw on strong evidence—of any kind—for describing artifacts and their interactions with people in activities* (Schiffer 2008).

This chapter addresses some actual and potential contributions of ethnoarchaeology to the archaeology of science. Two case studies illustrate the process of modeling generalizations on the basis of behavioral data: one treats the manufacture of alabaster vases in Egypt, the other the use of obsidian hide scrapers in Ethiopia. This chapter also shows that additional scientific issues can be addressed through controlled comparisons among independent ethnoarchaeological observations. The case studies deal with (1) form–function relationships in traditional pottery and (2) explaining the adoption patterns of a technology spawned by scientific investigation. Finally, there is a template for gathering behavioral data in an ethnoarchaeological study of a modern laboratory.

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Modeling Recipes

Recall that a recipe is the researcher's model of the knowledge that allows people to carry out complex activity sequences (see chapter "Varieties of Scientific Knowledge"). The raw behavioral data needed for fashioning recipes are ubiquitous in the literature, for ethnoarchaeologists have observed and recorded the tools, materials, and interactions of diverse activities. Although ethnoarchaeological reports seldom contain every detail needed to model a recipe, the lacunae may be inconsequential or readily bridged.

Manufacture of Alabaster Vessels

Alabaster is a calcium carbonate mineral easily worked. From this white material the ancient Egyptians made vases of varied shapes and sizes, architectural elements, and sarcophagi. The craft, which ceased to be practiced more than two millennia ago, was revived, probably in the early nineteenth century (Hester and Heizer 1981). Hester and Heizer suggest that the revival coincided with the growth of tourism and a market for Egyptian antiquities that could be supplied with good forgeries.

Some alabaster workshops have in recent years been mechanized, using lathes to turn out vases. Fortunately, Hester and Heizer were able to visit two adjoining workshops in Gurna, a small settlement where alabaster vases were made by hand. Because of the two-millennia hiatus in working alabaster, modern artisans no doubt had to invent the manufacture process through trial and error, creating their own recipe.

Alabaster is quarried from outcrops about 50 km distant from Gurna. After being worked into "roughly shaped blocks" (p. 293) it is delivered by donkey caravan to the workshops and placed in a storeroom. At this point the manufacture process may begin.

My model of the vase-making recipe includes many quotations from Hester and Heizer (pp. 293–296) and adds several necessary interactions. The recipe is written as if it were to be read and followed by others, and so steps are ordered accordingly. Hester and Heizer also furnished images of tools, work in progress, and a map of the workshops; I found them to be very helpful.

1. Select "a block [of alabaster] from the storeroom" and carry it to the extramural work area.
2. Remove the tools and materials from the tool room and place them in the work area.
3. With "a short-handled double-pointed hammer" trim the block into the shape of a "preform," which approximates the vase's final shape.
4. In preparation for hollowing out the interior, cover the preform's exterior with melted glue, sprinkle on it alabaster dust, and then wrap it, mummy-like, with "strips of glue-soaked cloth." (This practice is said by the workers to help

"prevent breakage … during the hollowing-out process;" it is unknown whether wrapping the preform has this beneficial effect or is a ritual.)
5. Allow the wrapping "to dry and harden."
6. Carry out the "Initial hollowing … with a hand-held three-pronged iron chisel."
7. After creating a "fairly shallow concavity," coat the interior with "a concoction of glue and alabaster dust." (According to the workers, this concoction "goes inside the stone and hardens it," but it may be a ritual practice.)
8. Assemble the boring tool, which consists of a brace, two serrated, lunate-shaped iron "bits," and several iron wedges that secure the bits. (The boring tool is illustrated, p. 294, which greatly clarifies the bits' shape and arrangement of parts.)
9. Scoop out a small pit in the workshop's earthen floor and "set the vessel in this."
10. With the assistance of a helper, begin the process of boring the interior, changing bits as needed "in order to make the inner contours of the vessel."
11. After the boring is done, remove the exterior coating with "the short-handled pick-hammer."
12. Drive some pegs into the workshop floor and brace the vessel against them.
13. Hold the vessel in position with your feet and, with a steel file, give the exterior its final shape, turning the vessel periodically.
14. With an unshaped chunk of sandstone, smooth the striations left by the filing.
15. Using "a piece of coarse commercial sandpaper," continue the smoothing process.
16. Place the vessel in "a bread oven in the nearby ramada" and heat it "for about 5 min."
17. With the vessel still hot from the oven, apply "Alexandria Wax" to all surfaces, which will turn the alabaster from white to light brown.
18. For the final polish, rub the vessel "with a piece of cloth."
19. The vase, now complete, may be carried to the display room.

The reader no doubt sees that this recipe contains several incompletely described materials and interactions. There is no information about the kind of glue or cloth used, the grades of file and sandpaper, the precise forms of the "short-handled double-pointed hammer" and "three-pronged iron chisel," the amount of pressure to apply when boring, and so forth. And we do not know how hot the bread oven was before the vase was inserted and whether gloves were used to remove it. Even so, the above model of the recipe, which employs all the information that Hester and Heizer supplied, would give a replicator, willing to engage in trials to fill in details and to acquire skill, sufficient information to transform an amorphous chunk of alabaster into a handsome vase.

Processing Hide with an Obsidian Scraper

A common archaeological lament is that few groups today make and use chipped-stone tools in traditional tasks; even rarer are stone tools retouched during use.

However, *few* does not mean none, and Clark and Kurashina (1981) present a case study of specialist tanners in central Ethiopia whom they observed using—and retouching—obsidian scrapers.

The scraping tool consists of three parts: (1) a unifacially flaked ovate or "sub-rectangular" scraper of obsidian, (2) a wooden haft, about 20–30 cm long, shaped like the eye of a needle, which accommodates two scrapers on opposite sides of the wider end, and (3) a mastic derived from a tree resin for attaching the scrapers at a fixed angle (110–120°) in relation to the long axis of the haft.

Drawing on Clark and Kurashina's description of interactions, I have constructed the following model of the recipe for using and retouching the hide scraper.

1. Buy a hide, wet or dry.
2. Peg out a wet hide for drying.
3. Place a very dry hide flat on the ground, wet it, and then pound it "with a stone to smooth it and flatten out the creases" (p. 305).
4. Inside the work hut, place the pole frame in a vertical position.
5. Fasten the hide firmly to the top of the frame.
6. Attach several ropes to the bottom of the hide, allowing the free ends to rest on the floor.
7. Stand on the ropes so as to hold the hide reasonably taut.
8. Grasp the hollow part of the haft in one hand, and place it on the hide where fat and other tissues remain.
9. With the other hand apply pressure to the end holding the scraper as you draw it downward.
10. Moisten the place where you want to scrape next, by drawing into your mouth some water from a nearby container and blowing it on the hide.
11. Repeat steps 8–10 about 15–20 times, after which the scraper will require resharpening.
12. Retouch the scraper by "lightly flaking" it with an "iron fabricator" (p. 308).
13. Repeat steps 11 and 12 until the hide is clean.
14. When you have finished cleaning the hide, "the two mounted scrapers are exhausted," their edge angles too steep for further retouching and use (p. 306). Replace the worn-out scrapers by heating the mastic, removing the old ones, and inserting fresh ones.

Some details are missing from Clark and Kurashina's (1981) account, including how a wet hide is pegged to the ground, the design of the frame, how the hide is attached to the frame, the size and shape of the pounding stone, and the form of the "iron fabricator." Even so, by means of this recipe and diligent trials one could learn how to use and retouch the scraper.

Discussion

These case studies demonstrate that ethnoarchaeological reports of people–artifact interactions furnish evidence for modeling recipes. Reports may contain gaps in

behavioral descriptions, but gaps can be filled with justifiable inferences. In any event, recipe models appear to approximate the knowledge that artisans possess for making and using artifacts.

It would be instructive to learn more about how people adapt recipes to new circumstances. What happens, for example, when an ingredient for a meal was no longer available? Ancient cooks could not surf the Internet looking for substitution possibilities, so what did they do? We could find out by scouring old cookbooks and other literature, seeking patterns in how recipes for traditional cuisine changed during population movements, such as the dispersal of Jews to North Africa, India, China, Iran, and so forth after their expulsion from Spain during the Inquisition.

Controlled Comparisons

The previous chapter discussed controlled experiments, which help to illuminate generalizations deeply embedded in a class of technologies found in many societies. The counterpart in ethnoarchaeology to the controlled experiment is the controlled comparison. Here, the researcher obtains data on similar science-related activities as reported in a sample of societies. The cases chosen for a comparative study usually conform to explicit parameters such as "dwellings built by groups having high residential mobility" or "apparatus with which tribal farmers make astronomical observations."

This section presents two case studies: (1) form–function relationships in the pottery of traditional societies and (2) patterns in the eighteenth-century adoption of the lightning conductor, a science-generated technology.

Form–Function Relationships in Ceramics

Controlled comparisons can elicit, for example, experimental laws and empirical generalizations that relate an artifact's form, such as shape and size, to its expected (or potential) function. For example, a clay cooking pot for boiling food "must have a mouth large enough to prevent explosive boiling over and to permit of stirring its contents, but at the same time small enough, relative to the pot's capacity and heating surface, to prevent it from boiling dry every few minutes" (Linton 1944:370). Since Linton's seminal paper, which was based on a comparative study of North American cooking pots, archaeologists have investigated form–function relationships and set forth numerous generalizations (among these studies are Henrickson and McDonald 1983; Smith 1985; for additional references, see Rice 1987, chapter 7; Skibo 2013, chapter 2; see also Braun 1983; Roux 2007).

A generalization typical of recent research is the following: "Long-term dry-storage vessels are usually tall and proportionately rather thin," tend to have "an opening wide enough to scoop from," and "almost all … have rolled-over or everted

rims" (Henrickson and McDonald 1983:632). This generalization identifies several formal properties that converge on vessels used in this way, expressing a cross-cultural pattern. The selection of the ethnoarchaeological sample cases (by Henrickson and McDonald and others) has necessarily been opportunistic, for the researchers must search reports, old and new, seeking relevant data. Accordingly, the resultant statistical generalizations (usually expressed as a percentage of conforming cases) can be extrapolated to a larger population only with great care.

Despite shortcomings, the generalizations arising from controlled comparisons play an important role in archaeological inference, for they allow us to generate plausible hypotheses about the expected function of a specific *class* of prehistoric vessels defined on the basis of morphology (e.g., Smith 1988). Clearly, *individual* vessels may have complicated life histories, such as multiple and sequential functions, but that does not obviate the usefulness of statistical generalizations about a *vessel class*. Moreover, functional hypotheses may be tested with use-alteration analyses that implicate *actual* functions (Skibo 2013).

The generalizations disclosed by controlled comparisons had to be learned by past peoples and passed down, through practice, in a technological tradition. Linton (1944:370) observed that traditional "potters must have discovered these facts (i.e., generalizations) through experience, and adjusted their designs to them." Indeed, various discovery processes (see chapter "Discovery Processes: Trial Models"), especially trial and error, would have led the earliest makers and users of pots—and other technologies—to arrive at forms that, by virtue of their interaction- and activity-specific performance characteristics, could carry out the expected functions. Otherwise, the new technologies would not have been reproduced. When different groups adopted the new technologies, they necessarily adopted as well the implicit generalizations. Thus, the form–function generalizations created by ethnoarchaeologists closely model the kinds of scientific knowledge that informed the design process and, consequently, resulted in past technologies (on design and development, see Schiffer 2011, chapter 8; Schiffer and Skibo 1997; Skibo and Schiffer 2008).

Discussion

The pottery example deals with generalizations about utilitarian functions, but scientific generalizations related to symbolic and emotive functions—also learned through experience—frequently influence artifact design (see Schiffer 2011:102–106).

Form–function relationships are the most familiar kind of generalization. Another example is Dean Arnold's (1985) statistical generalizations that describe how far traditional potters travel to obtain their materials. Not surprisingly, they travel short distances to obtain clay, but go farther for temper and much farther for paint. Arnold's controlled comparisons were made possible by the plethora of relevant data in reports on pottery-making societies.

Patterns in the Adoption of a Science-Generated Technology

A previous chapter, "Science: A Behavioral Perspective," has argued that every technology-development project creates new science in the form of recipes. In turn, every new recipe embodies descriptions and generalizations hard-won by its creators. In this trivial sense all technologies can be considered "applied" science. Moreover, through subsidiary technology projects, science projects may lead to new technologies (along with corresponding recipes and the generalizations upon which they depend). Sometimes a new technology arises from a science project as the straightforward implication of a theory or experimental law. One such technology is the lightning conductor, an outgrowth of Benjamin Franklin's experiments on atmospheric electricity. The lightning conductor was applied science in a *nontrivial* sense because this technology was unlikely to have arisen in a context other than a science project aiming to enrich human understanding of environmental phenomena.[1]

I propose that the archaeology of science include research on the adoption processes of science-generated technologies. Franklin's lightning conductor is an especially intriguing case study because other investigators, namely Jean-Antoine Nollet in France and Benjamin Wilson in England, disputed Franklin's claims for its efficacy (Schiffer, Hollenback, and Bell 2003).

The lightning conductor, commonly called a lightning rod, protects a structure by passing the charge from a lightning strike harmlessly into the ground (or in the case of ships, into the sea). In addition to the rod on the roof, the lightning conductor includes fixtures to hold the rod in place and fasteners to secure the wire as it wends its way into the ground. Franklin argued that lightning conductors should be tipped by points, but Wilson insisted on the basis of his experiments that a rounded rod or ball tip was preferable. Nollet went much further, arguing that the lightning conductor in any configuration endangered life and property because it would attract lightning.

Franklin publicized his new technology beginning in 1750, and it was rapidly commercialized on both sides of the Atlantic. Lightning conductors were advertised in instrument catalogs, but most installations would have been customized for each structure or ship and priced according. For many people the high cost of custom conductors would have discouraged adoption.

Although the eighteenth century lacked ethnoarchaeologists, Marsilio Landriani, a professor of physics and a persuasive advocate of Franklin's invention, published in 1784 a set of data on 323 European installations of lightning conductors. These data were apparently obtained through correspondence in a rudimentary kind of survey research. For each entry—an installation—Landriani (1784:285–304) reported the kind of structure, its location, and usually the owner's name. Thus, his

[1]The remainder of the case study is adapted from Schiffer (2011:160–162) and Schiffer, Hollenback, and Bell (2003:200–205).

data meet the conditions necessary for conducting a controlled comparison among adopters of lightning conductors. I performed a simple analysis of these data and teased out major adoption patterns.

The first task is to consider the gaps and biases in Landriani's data, which necessarily constrain the conclusions. The most entries—120—come from Landriani's home country, Italy, followed by France and Germany with a little more than 60 each; there is a mere sprinkling of installations elsewhere. Given the plurality of Italian adoptions, these data most likely reflect observations and activities of people in Landriani's social network. In view of this geographical bias, we can draw no accurate conclusions about total adoptions or differences in adoptions among countries. However, one conclusion does stand out: adoptions of the lightning conductor, even in Italy, were sparse.

Despite the data set's shortcomings, aggregated data reveal patterns in the kinds of structures protected as well as in the purchasers' social positions. Houses, by far the most abundant structure in almost all communities, make up little more than half the installations (54%), followed distantly by religious structures (14%), palaces and castles (8%), military structures (7%), and public buildings (6%); schools and factories together make up only 3%.

Setting aside houses for the moment, we see that the vulnerable structures of organizations, especially wealthy ones, were more likely to be protected. As stewards of such properties, church and state functionaries were apt to have in their ranks people familiar with electrical matters. Because many electrical experts were clergymen, they would have advocated the outfitting of religious structures. No doubt many castles, palaces, and military installations in wealthier countries were protected, whereas poor parish churches were not. I suggest that access to electrical expertise and sufficient wealth were two factors favoring the acquisition of lightning conductors for nonresidential structures.

Residential structures followed a similar adoption pattern. I divided houses into two groups: those owned by a titled person, such as a duke or earl, and all others. In any community the nobility would be a minuscule minority of all residents, yet titled persons owned nearly half the houses—84 of 174. What is more, many other houses, some listed as "country homes," doubtless belonged to the elite. Among the untitled adopters were scholars and natural philosophers, many with knowledge of electricity. The inescapable conclusion is that residential lightning conductors were adopted mainly by small numbers of elite consumers.

We may also conclude that risk assessments, however flawed and implicit, probably figured in adoption decisions, as indicated by nonresidential patterns. Many people had learned through experience that tall buildings and those in high places were at greater risk than other structures, and so churches, palaces, and castles were more likely to be outfitted with lightning conductors than other nonmilitary buildings. In addition, the most highly protected type of building, in all countries for which some data are available, was the powder magazine. The dire consequences of a lightning strike on a powder magazine were well known after the 1769 Brescia disaster, when lightning set off the entire store of an unprotected magazine, leveling

the town and killing some 3,000 people. We may thus conclude that risk assessments entered into adoption decisions.

In all cases of adoption, people apparently decided that the new technology's ability to safeguard structures from the scourge of lightning clearly outweighed its cost. Nonadopters—even if they were wealthy and familiar with the science of lightning conductors—apparently favored parsimony over protection, perhaps having concluded that the risk of a lightning strike on their structure was negligible.

The lightning conductor is an unusual science-generated technology because it could be adopted by any wealthy consumers interested in protecting their properties. In contrast, many modern science-generated technologies, such as cyclotrons and gene sequencers, have been adopted mainly by specialized laboratories. Others, such as the electromagnet, were incorporated into products such as motors that, only then, could be acquired by diverse consumers. Regardless of the composition of the consumer group, science-generated technologies offer many opportunities for research on adoption processes.

Discussion

The ethnoarchaeological, ethnographic, ethnohistoric, and historical records contain vast amounts of behavioral data that can be put in the service of controlled comparisons. Two case studies have illustrated the extremes of this process: the one on form–function relationships depended on ethnographic and ethnoarchaeological evidence, the other on the adoption of a science-generated technology relied on historical evidence. Yet both data bases supplied behavioral data relevant to well-defined research problems in the archaeology of science.

Template for Gathering Data in a Modern Laboratory

The ethnoarchaeology of a science project in a modern laboratory offers a splendid opportunity to monitor many activities and people–artifact interactions. A few scholars have done ethnographic research in laboratories (e.g., Goodfield 1991; Latour and Woolgar 1979; Traweek 1988), but I am unaware of any ethnoarchaeological study. I anticipate that some archaeologists will move in this direction, and for them I supply the following template to guide the gathering of basic behavioral data in the service of varied research problems.

1. Is the laboratory part of a corporation, university, or government agency?
2. What is the nature of the project and its disciplinary context?
3. What is the expected outcome(s)?
4. What resources are necessary for pursuing the project?
5. Of what spaces and places does the laboratory consist?

6. What apparatus and supplies are found in the laboratory?
7. Where in the laboratory's spaces are the apparatus and supplies located, and what are their relationships to facilities and utilities such as benches, storage facilities, sinks, lighting, and electrical outlets?
8. How are the apparatus obtained (legacy of earlier projects, purchased on a grant or contract, received as a gift, etc.)?
9. Is the laboratory home to several related projects? If so, which apparatus are shared and which ones are dedicated to the target project?
10. Who are the project's participants?
11. What is the project's social organization?
12. Does the project have an ideology?
13. What are the project's frequent and infrequent activities, including rituals, and where do they take place?
14. What performances of people and/or apparatus cue the initiation of specific activities?
15. In what ways, and how often, do the project's participants interact with the apparatus in those activities?
16. What skills are required to operate the apparatus and how do participants acquire those skills?
17. Are materials, apparatus, personnel, or information exchanged with other laboratories?
18. Are unexpected obstacles encountered? If so, do they lead to changes in the project's activities, apparatus, or personnel?
19. What was the project's outcome(s) and did it accord with the one(s) originally envisioned?

The above questions, when pruned or augmented, can supply fine-grained behavioral data for problem-oriented ethnoarchaeological research. For example, if I were interested in how social power is exercised in coordinating the project's activities, I would privilege information about social organization, social roles, and on how specific activities are cued. Likewise, if I had an interest in identifying factors that affected the project's design, I would gather data on how its activities are distributed over time and space.

As ethnoarchaeological studies of laboratory-based projects accumulate, we may be able to undertake a generation of controlled comparisons that illuminate the sources of intra- and interdisciplinary variation in investigations.

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Contributions of Archaeometry

Archaeometry is the application of physical and biological science expertise and technologies to archaeological materials (Bowman 1991; Killick 2008; Malainey 2011); its host of subfields range from archaeometallurgy to zooarchaeology. In modern archaeology, archaeometric analyses are a necessity in most projects because they furnish unique evidence for inferring past human behavior, societal organization, and environmental contexts. Archaeometry is also a prolific contributor to the archaeology of science, as many studies provide information for modeling generalizations.

Ideally, archaeometry is practiced by people trained in both archaeology and another science. However, archaeologists also collaborate with outside specialists, especially when there is a need—as in the modeling of recipes—to employ instruments of modern chemistry and physics for characterizing materials at the atomic, molecular, or crystalline levels. To illustrate archaeometry's role in modeling recipes, I turn to Maya blue, a very unusual pigment, and cylinder jars from Chaco Canyon, a rare vessel form; the former illustrates recipes of manufacture, the latter recipes of use.

Maya Blue, the Mysterious Pigment

On murals, painted sculptures, and pottery from the Classic and Postclassic periods of the Maya, a prehispanic civilization of lowland Mesoamerica, archaeologists had noted a lovely blue pigment, now called Maya blue, which occurs mainly in ritual contexts. Questions thus arose: what is the composition of this exquisite pigment and how was it made? Although research on Maya blue began in the early twentieth century, owing to the small samples available, instrumental limitations, and lack of a large-scale project, it took many decades for specialists employing a variety of characterization techniques to piece together the answers. So unexpected were the answers that Maya blue continues to attract the interest of chemists and materials scientists.

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An obvious hypothesis was that the pigment had been made from a blue copper mineral such as ultramarine or azurite, but chemical analysis of Maya blue found no copper (Merwin 1931). In further analyses, chemist Gregory P. Baxter identified aluminum, silicon, calcium, and iron, all common constituents of clays (Gettens 1962). Among these elements only iron compounds can impart a blue color, and so it was suggested that Maya blue owes its hue to an iron silicate. Visual comparisons of Maya blue with iron silicate minerals undermined this hypothesis (Gettens 1962).

Anna O. Shepard, a ceramic scientist at the Carnegie Institution who worked closely with archaeologists, undertook petrographic analyses of Maya blue specimens from Mayapán in the mid-1950s. She found that "The blue material appears in flocks lacking a definite particle size and shape … The flocks appear to be composed of minute irregular flakes or plates of the order of 0.01–0.02 mm in length and having a random arrangement" (Shepard, quoted in Gettens 1962:558). But she could not identify the color's source. About the same time Rutherford J. Gettens, a chemist and pigment authority at the Smithsonian's Freer Gallery of Art, determined through X-ray diffraction that the clay in Maya blue is the mineral attapulgite (now known as palygorskite), which occurs in the Maya area (Gettens 1962). Although this finding was crucial for modeling the recipe, attapulgite is white not blue.

Gettens assessed what was known as of 1961: "Although we have in the last 30 years extended our knowledge of the nature and range of use of Maya blue we still have not identified the blue colouring principle" (Gettens 1962:557). Rediscovering some vials of pigment collected decades earlier from the Yucatan, presumably from modern Maya Indians, Gettens discerned that one of them, *Azul de Tekax*, was identical to Maya blue and contained "a blue organic dyestuff which has the properties of indigo" (p. 560), and can be obtained from species of the plant genus *Indigofera*. It was known that indigo could be degraded by nitric acid, but not so Maya blue. This left Gettens with two possibilities: is the Maya blue in the sample of *Azul de Tekax* "of modern origin fortified with indigo or is it possible that the blue component of Maya blue is indigo?" (p. 560). At the time, it seemed highly unlikely that a stable pigment could be made from attapulgite and an *organic* material such as indigo.

Seeking further information on the properties of Maya blue, Gettens (1962:561) scraped small samples from a prehistoric incensario and immersed them in "concentrated nitric acid, concentrated hydrochloric acid, aqua regia, concentrated sulfuric acid, 5 % sodium hydroxide." The results were astounding: even after 18 h, the color was unchanged. On the basis of these tests Gettens concluded "that the blue is an integral part of the mineral base, not a superficially attached dye" (p. 562). That no organic colorant could seemingly survive these harsh tests allowed a further tentative conclusion: "Maya blue is an *inorganic* pigment" (p. 562, emphasis mine).

In a brief note immediately following Gettens' paper, Shepard (1962) assessed several hypotheses about the pigment's composition. Systematically discounting the inorganic alternatives, she proposed the radical hypothesis that "Maya blue is a clay-organic complex with an organic colorant in an inorganic base of attapulgite clay" (p. 565). She also noted that attapulgite has an unusual structure for a clay mineral because its molecules are not plate-like but fibrous.

Shepard's unconventional hypothesis received support from the experiments of clay chemist van Olphen (1966). Employing attapulgite and both synthetic and natural indigo, he confected two recipes that yielded a complex comparable in color and chemical properties to Maya blue. The key to both recipes was the use of "less than 0.5 %" indigo and heating the powder "for several days at 75 °C or preferably at 105–150 °C" (p. 645); but how heating worked its magic was a puzzle. He also showed that Maya blue could not be made from typical platy clay minerals; the fibrous structure of attapulgite was necessary. He speculated that the indigo molecules "adsorbed on only the external surfaces of the [attapulgite] particles" (p. 646). However, van Olphen's simplified recipes required materials that the ancient Maya lacked: acetone in one, sodium hydrosulfite in the other. Nonetheless, van Olphen increased the likelihood that Maya blue was, as Shepard had boldly proposed, a "clay-organic complex," but details of the ancient Maya recipe were still to be learned.

Chemical tests and X-ray diffraction analyses carried out by previous workers were summarized, critically evaluated, and repeated by Edwin R. Littmann (1980). A medical doctor with chemistry expertise, Littmann had access to X-ray diffraction equipment at an Exxon laboratory. While confirming the basic findings of earlier studies, he also pointed out that indigo had not been found in *archaeological* specimens of Maya blue; presumably its low concentration fell below the detection limits of X-ray diffraction. He also observed on museum specimens variation "from bright blue to gray-blue, verging on green" (p. 87), which hints at the use of multiple recipes.

Littmann (1982) later turned to replication, which he began by extracting the indigo dye from the plant. He pointed out that the source of indigo's color, indican, "is water soluble and easily extracted by steeping the leaves and stems in warm water" (p. 405). In a series of eight experiments, Littmann tested both synthetic and natural indigo, employing several heating regimes. The natural indigo was extracted from *I. suffriticosa* by intermittently boiling its leaves in water; the extracts were mixed with heated attapulgite, boiled, and stirred while cooling. In several trials he obtained various shades of blue, but only after adding a small amount of dilute hydrochloric acid. The ancient Maya, he concluded, could have used different acids. Littmann's replications provided several plausible but very general recipes for processing the indigo and combining it with attapulgite to produce a stable blue pigment in several shades.

Torres (1988) exacerbated the equifinality problem by drawing on Colonial Period sources that reported the production of blue pigments in the Maya area, but he carried out neither replications nor analyses of ancient Maya blue. In any event, variations on the recipe seem rather minor, and we may suppose that the ancient Maya employed some of the variants, especially those yielding different shades of blue. Reyes-Valerio (1993), also drawing on early historic accounts of indigo production in Central America, argued that the clay for making Maya blue was not quarried but was present, as a suspension, in the water used to soak the indigo leaves. He quotes ethnohistoric documents specifying that the water is to be obtained from turbid streams or arroyos, which naturally contain suspended clay particles. That such a process can yield a pigment having the properties of Maya blue seems doubtful.

As mentioned above, chemical tests had shown Maya Blue to be extremely stable, able to resist attacks by strong acids and bases. According to Chiari, Giustetto, and Ricchiardi (2003:21), Maya Blue was "the most stable pigment ever produced." In view of its unique properties, chemists and others were interested in determining its molecular structure. Decisively resolving one of Littman's concerns, infrared spectroscopy and photoluminescence spectroscopy "unequivocally revealed the presence of indigo in both the antique pigment and in the synthetic one" (Chiari, Giustetto, and Ricchiardi 2003:22). This finding complemented detailed microstructural studies that revealed how the indigo was chemically bound in the clay's crystal structure. Using powder synchrotron diffraction and molecular modeling, Chiari, Giustetto, and Ricchiardi (2003) showed that moderate heating of the clay enabled indigo to create the necessary chemical bonds in the palygorskite's fibrous channels (see also José-Yacamán et al. 1996). Through experiments and optical spectroscopy, Reinen, Köhl, and Müller (2004) demonstrated that indigo molecules were incorporated into the clay's crystal lattice. Beyond reaffirming that Maya blue required both indigo and palygorskite, these highly technical studies have been of more interest to chemists than to archaeologists. This conclusion reminds us that archaeological finds can sometimes challenge conventional knowledge in the physical and biological sciences. Indeed, it is doubtful that any chemist or materials scientist could have predicted on the basis of Maya blue's extraordinary chemical properties that the colorant was organic.

Drawing on previous archaeometric studies, ethnohistory, and archaeology, Arnold et al. (2007) offered a creative reconstruction of Maya blue manufacture. Their study was prompted by the rediscovery, in Harvard's Peabody Museum, of a large bowl recovered early in the twentieth century from the Cenote of Sacrifice at Chichen Itzá. The bowl retained its contents, which were predominantly copal, a tree resin that the Maya used as incense, and patches of white and blue substances. Scanning electron microscopy and energy-dispersive X-ray spectroscopy identified the patches as palygorskite and indigo, but there was no trace of Maya blue.

Despite the lack of Maya blue in the analyzed samples, Arnold et al. suggested that the pigment was made by burning copal in the presence of palygorskite and indigo. In support of this conjecture they furnished evidence that all three substances had been used for healing, and if burned together could have produced smoke as an offering to the rain god *Chaak*. They also proposed that "the ritual combination of three materials used for healing suggests that the actual performance of the creation of Maya Blue was significant and might have had great symbolic value critical to the meaning of the pigment" (p. 154).

The recipe for making Maya blue implied in this scenario raises troubling questions. Could the burning of copal containing *separate* patches of palygorskite and indigo have yielded Maya blue? On the basis of everything we know about Maya blue, this seems impossible. But if the indigo and palygorskite had been thoroughly mixed together and suspended in molten copal (melting point below 150 °C), could Maya blue have been produced? At this point, we do not know. Even if Maya blue had been made in this way, how could it have been extracted from the copal for use in a paint?

Questions aside, Arnold et al. (2007) convincingly argued that Maya blue was used in rituals, pointing out that near the bottom of the Cenote of Sacrifice there is a thick blue layer, apparently composed in part of Maya blue that washed off artifacts and sacrificial victims (Arnold et al. 2007:157). And it also seems possible that, as Arnold and colleagues suggest, Maya blue was made in a ritual context. But what of this bowl and its contents? A hypothesis respecting the ritual significance of the three materials is that the vessel containing symbolically charged ingredients was used in a performance, perhaps one that celebrated the manufacture of Maya blue, and was discarded afterward in an appropriate receptacle—the Cenote of Sacrifice.

In crafting a provisional recipe for making Maya blue, we ought to privilege the certainties while acknowledging the remaining uncertainties. Indeed, behavioral chain analysis (Schiffer 1975) teaches us that compositional evidence alone is an insufficient basis for modeling a *detailed* manufacture recipe. To refine a recipe, we must define activities in behavioral terms, identify the artifacts and ingredients that interacted in those activities, and specify the interaction sequences. Clearly, behavioral chain analysis allows us to bring into inferences a greater variety of evidence and interactions.

Some questions inspired by behavioral chain analysis are the following: if the indigo leaves and stems were macerated before being boiled, what tools and containers were used? Given that the palygorskite had to be ground finely before it was mixed with indigo, what tools were used? Was a decanted indigo solution used or was the water boiled off or allowed to evaporate, creating a chunk of indigo? If the latter, might another vessel have been needed? O'Neale (1945:28–29) reported that Guatemalan weavers received indigo from El Salvador in solid chunks that had to be ground to a fine powder. If the ancient Maya also made solid indigo, they would have employed grinding tools to create powder. One of the artisan's "trade secrets" could have been the ratio of powdered ingredients, by volume, for making predictable shades of blue. The measurement of dry ingredients implies standard units of volume, perhaps discernible in ceramic vessel sizes. With what tools were the ingredients mixed together? How was this mixture heated? These questions would lead us to examine a range of vessels, grinding stones, and other tools for use-alteration traces (e.g., residues, abrasions, chips—see Skibo 2013) and formal properties as well as performance characteristics that might be consistent with the posited interactions. We may also ask whether additional ingredients were needed to make the final paint and how the paint was applied. And, of course, there are questions about the gendering of activities, how the manufacture process was organized, and what was exchanged—ingredients, solid pigment, or paint.

A trial model of the Maya blue recipe is as follows:

1. Acquire a quantity of leaves or leaves and stems from *I. suffriticosa*, a species common throughout Central America.
2. Acquire a quantity of palygorskite clay from deposits in the Maya area.
3. Grind the clay into a fine powder and remove such foreign matter as twigs and pebbles.
4. Put the plant parts and a quantity of water in a ceramic vessel.

5. Place the vessel over a fire and boil its contents for several hours; allow it to cool.
6. Pour the liquid indigo into a separate vessel and allow it to dry or boil off the water.
7. Mix 100 parts powdered clay with less than 1 part indigo, and heat the mixture for several hours at 105–150 °C.
8. Perhaps add a small amount of acetic acid, which can be obtained from any spoiled alcoholic beverage.
9. The pigment is now ready to be made into paint.

Clearly, there is much potential to conduct further replications using only materials, water, and tools available to the Maya. Regardless of how such experiments turn out, we know—thanks to archaeometric studies—that the ancient Maya developed one or more relatively simple recipes to make an extraordinary material. Indeed, Maya blue continues to be intensively studied by physical scientists be-cause it is a "nanostructured polyfunctional hybrid organic–inorganic material" (Doménech et al. 2009:2371) of a kind new to modern science.

Cylinder Jars of Chaco Canyon

Besides modeling manufacture recipes, we may model recipes for any process along an artifact's behavioral chain (see chapter "Varieties of Scientific Knowledge"). This case study focuses on a recipe for the use of cylinder jars, an unusual ceramic form recovered mainly from ancient pueblo sites in Chaco Canyon, northwest New Mexico. A use-related recipe specifies the interaction sequences in the activities that took place between manufacture and cultural deposition. It describes other interactors that participated in each activity, identifies the composition of the social group, and spells out the timing and locations of activity performance. And, to the extent possible, it contextualizes these activities in relation to relevant societal processes. In our present state of knowledge the recipe for cylinder jar use includes many lacunae and untested hypotheses, which invite more research.

Chaco Canyon was the focal point of a farflung, prehistoric regional system, consisting of a dozen or so very large pueblos and hundreds of small ones (Vivian and Hilpert 2012). Many products were exchanged in this system, often traveling great distances. Indeed, the canyon itself, where many of the large pueblos are located, was a land of scarce resources with insufficient wood to fire much pottery. And so ceramics were acquired on a large scale from distant villages.

Cylinder jars are very rare: in his comprehensive inventory, Toll (1990) counted a mere 210 examples. Moreover, more than half—111—were found in Room 28 at Pueblo Bonito (Fig. 1), a site where two major but early excavations were carried out (Judd 1954; Pepper 1996). The largest site in Chaco Canyon, Pueblo Bonito consisted of more than 600 masonry rooms and was occupied during the Bonito Phase from about 900 to 1150 C.E. (a recent work on Pueblo Bonito is Neitzel 2003). The cylinder jars were likely made and deposited during the late eleventh

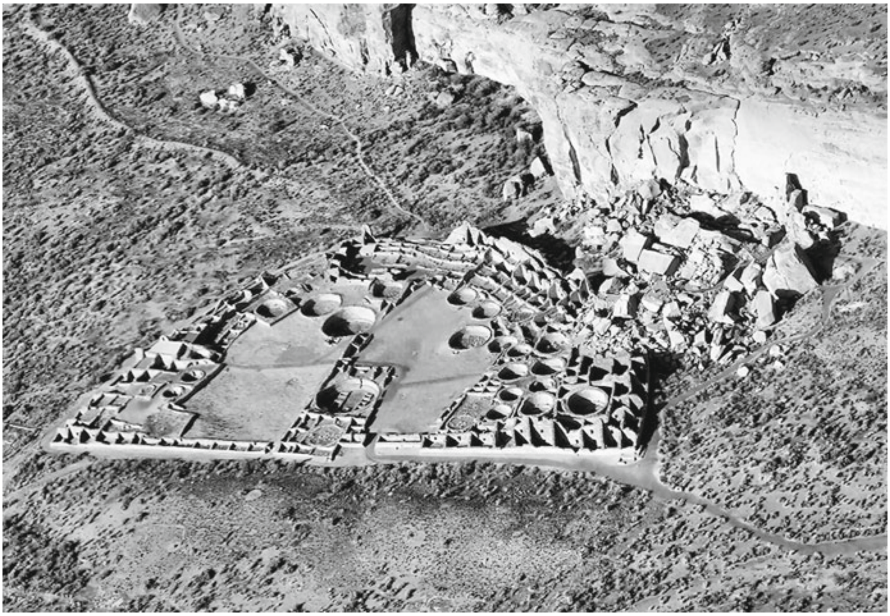

Fig. 1 Pueblo Bonito, Chaco Canyon, New Mexico (Wikimedia Commons; Bob Adams photographer)

and early twelfth centuries (Toll 1990:285). Recovered mainly from caches and burials, the vessels tend to be whole or nearly whole, permitting comprehensive metrical descriptions (Toll 1990; Washburn 1980).

According to Toll's (1990) tally, the vast majority of cylinder jars are either plain white (51) or black paint on a white slip (82); only four are redware and these tend to have somewhat different attributes. Almost all cylinder jars have three or four tiny lugs—called loop handles or just handles—placed around the rim at various distances below it (Washburn 1980). Toll illustrated 79 cylinder jars and summarized major attributes of more than half the known examples ($n = 120$–137), including mean height, 23.7 cm; mean neck diameter, 10.8 cm; mean base diameter, 10.4 cm (see also Washburn 1980). In all attributes, however, the ranges are large as is the variation in painted designs, rim and vessel profiles, placement and orientation of lugs, and the potters' skill levels (Fig. 2). As Toll suggests, and I concur after scrutinizing his images, this variability indicates manufacture by many potters (most of whom likely resided outside Chaco Canyon).

On the basis of a close examination of a sample of cylinder vessels ($n = 16$) in several museums, Crown and Wills (2003) showed that some (5 certain; 3 uncertain) underwent reslipping, repainting, and refiring. This practice, they argued, is evidence of a renewal ceremony, part of a larger pattern that included extensive architectural refurbishing and remodeling. In support of this interpretation, they cite ethnographic accounts of renewal ceremonies in the American West, Southwest, and Mesoamerica.

Fig. 2 Chacoan cylinder jars exhibit considerable variation (Courtesy of the American Museum of Natural History Library, Image 3521)

Cylinder jars are known from Mesoamerica (Washburn 1980), which also supplied the American Southwest with copper bells, scarlet macaws, and—much earlier—maize, beans, and squash. Clearly, there has been intermittent long-distance interaction, probably down-the-line exchange, between the Southwest and Mesoamerica, but Mesoamerica did not supply the cylinder jars. Expressing a growing consensus, Toll (1990:286) remarked that "There is little doubt that the white ware cylinder jars were made in the San Juan Basin and decorated in styles of their time and place, but the use of other Mesoamerican items in special contexts, the significance of the [vessel] form in Mesoamerica, and the distribution of cylinder jars makes some symbolic link to Mesoamerica seem probable." There is also a consensus that these vessels played a role in ritual activities.

Toll (1990:295) suggested that cylinder jars all had the "same function," a justifiable hypothesis in view of the uniqueness of the form, its restricted distribution in time and space, and its deposition in special contexts. However, Washburn (1980) found a pattern in the orientation of lugs: on decorated vessels they tend to be horizontal, on undecorated ones vertical. This suggests that decorated and undecorated vessels might have differed in function(s).

Objects taking part in rituals often have utilitarian functions as well as symbolic and emotive ones. Crown and Wills (2003:513) list several previously hypothesized functions for cylinder jars: (1) "altar paraphernalia, perhaps to hold prayer sticks," (2) "storing luxury items such as shell and turquoise," and (3) "ceramic drums." The drum hypothesis, probably inspired by the lugs, seems far-fetched because of the puny sound that would have emanated from a small, closed vessel. Thus, we may

presume that the jars contained something during its preparation, storage, or use. But what was that something?

This question was answered in 2009 after an archaeometric study conducted by archaeologist Patricia Crown in collaboration with W. Jeffrey Hurst, Principal Scientist at the Hershey Center for Health and Nutrition (Crown and Hurst 2009). Hurst is an authority on the use of cacao in Mesoamerica who had previously collaborated with archaeologists (e.g., Henderson et al. 2007; Powis et al. 2002). Their plan was to seek cacao residues preserved in the vessels' walls, but they did not sample whole vessels. Rather, they analyzed sherds obtained during the recent re-excavation of old trenches in Pueblo Bonito trash mounds. The sample consisted of five sherds from five vessels: "Three were characterized as probable cylinder jars, one as a definite pitcher, and one as an indefinite cylinder jar/pitcher" (Crown and Hurst 2009:2110).

Although cacao (*Theobroma cacao*) contains more than 500 compounds, theobromine is diagnostic "because *T. cacao* is the only Mesoamerican plant that contains theobromine as the primary methylxanthine" (p. 2111). Employing high performance liquid chromatography and mass spectrometry, they analyzed the soluble fraction of the residue samples. The results were definitive: the three sherds of probable cylinder jars exhibited mass spectra characteristic of theobromine. On this basis the authors stated that "This is the first recovery of cacao in a Prehispanic context north of the Mexican border" (p. 2111).

The finding that cylinder jars contained cacao, whose ground seeds or pods can be used to make a liquid having a mild stimulative effect, buttresses the inference that the recipe for using the vessels includes interactions in ritual activities. But many questions remain. How was the cacao prepared? Did the recipe contain other ingredients, as in Mesoamerica? Further compositional analyses of a larger sample are clearly in order, perhaps stratified on the basis of undecorated, decorated, and redecorated vessels. Researchers might also seek theobromine residues in other vessel forms and grinding stones.

The images of cylinder vessels reproduced in Toll (1990) permit additional hypotheses about interactions during use. The absence of sooting shows that they were not used over a fire. The lugs strongly suggest that the vessels were suspended, most likely by cordage during storage or use (Toll 1990). Perhaps they were hung in a place protected from inadvertent contact or carried in a ritual performance. Washburn (1980) states that the lugs show no evidence of wear, indicating to her that the vessels were not hung or suspended. Her observations, however, predated the establishment of relevant generalizations for studying ceramic abrasion (i.e., Schiffer and Skibo 1989). Some evidence to evaluate these conflicting hypotheses may be acquired through a comprehensive use-alteration analysis of vessels. Washburn (1980:82) also observed broken lugs and wear on "some" jar interiors, which require further study. Use-alteration analysis might also enable testing of the hypothesis that redecorated vessels were "heirlooms" (Crown and Wills 2003:525); they ought to exhibit more wear. Clearly, quantitative inferences from wear patterns will require an intensive analysis. I note that if the vessels had been used to ferment a cacao product, the interior surfaces should be heavily eroded, a wear pattern sometimes confused with abrasion (Skibo 2013).

Although a cacao drink had been present in cylinder jars, we still do not know if it was being prepared, stored, or consumed; and the vessels may have had all three functions. Surprisingly, the vessel interiors are unslipped, which would have rendered their surfaces permeable (Schiffer 1990), allowing fairly rapid absorption of their liquid contents, at least during early uses before the pores became clogged. If the wall remained permeable for some time, it might suggest that the precious drink was held only briefly.

Crown and Hurst (p. 2112) suggest that the Chaco chocolate drink was prepared and consumed in a manner similar to cacao use in Mesoamerica: "The likely association of cacao with cylinder jars at Pueblo Bonito suggests that knowledge concerning the proper preparation, serving, and consumption of cacao beverages accompanied the seeds from Mesoamerica." But is this inference sustainable? The uses of cacao in Mesoamerica varied regionally and during the 2000-plus years that it was consumed prehistorically (e.g., Beliaev, Davletshin, and Tokovinine 2010; Green 2010; Powis et al. 2002). Which time and place, if any, would have been the source of recipes for the Chacoan practices? Also, in Mesoamerica several vessel forms were employed, including a spouted jar. These questions place a premium on expanding the study of residues on ceramics and other Chacoan artifacts.

A more complete recipe for cylinder jar use cannot be constructed at this time, but some hypotheses can be offered. It is very likely, as Crown and Hurst (p. 2112) suggest, that the cacao drink "was consumed by only a small portion or subset of the population, perhaps ritual specialists or the elite." But did consumption of this drink, containing a rare ingredient imported from more than 2,000 km away, take place in a private space such as a ceremonial structure or in a public place such as a plaza? Like the residents at many large pueblos in the American Southwest, Chacoans faced a daunting social problem, the integration of communities composed of immigrants from many places; this would have favored public ceremonies to promote integration. If cylinder vessels took part in such ceremonies, they may have been held or carried in a public display and their contents consumed during a ritual that celebrated and reinforced a pan-Chacoan social identity, perhaps performed seasonally as a rite of renewal (Crown and Wills 2003) and led by members of a sodality that drew its membership from many San Juan communities (cf. Toll 1990).

It has been suggested that the many cylinder jars in Room 28 at Pueblo Bonito were being stored there, perhaps removed periodically when needed in ceremonies (cf. Crown and Wills 2003; Toll 1990; Washburn 1980). Another possibility is that Room 28 was not used for temporary storage at all, but was an appropriate place for the *disposal* of ritual objects (Mills 2008). Because the contents of this cache may have represented the majority of cylinder jars that had been in use, at least at Pueblo Bonito, their deposition may indicate that the socioreligious activities that employed them had ceased to be conducted. Perhaps this cache was deposited while the Chaco regional system was collapsing. A less dramatic scenario, also based on the cessation of the rituals, is that cacao was no longer available, and so the cylinder jars were now without functions and had to be disposed of properly. In either case, the jars had reached "senescence" (Hollenback and Schiffer 2010). Radiocarbon dating of the cacao residue by accelerator mass spectrometry may supply evidence for eliminating one of these hypotheses.

In any event, the demonstration through an archaeometric study that cylinder jars once held a cacao drink is a stunning discovery with many implications for future research. Much remains to be learned about the vessel's use recipe, but it is likely that cylinder jars were employed publicly in integrative rituals, perhaps affirming a pan-Chacoan identity in a renewal ceremony. Additional studies employing archaeometry, use-alteration analysis, and other lines of evidence may lead to a more complete recipe.

Discussion

A previous chapter, "Varieties of Scientific Knowledge," asserts that empirical generalizations and experimental laws are embodied in all recipes and underlie specific interactions. A few simplified examples from the present chapter's case studies underscore this claim. In procuring materials to make Maya blue, artisans employed empirical generalizations that designated the locations in the environment yielding (palygorskite) clay and specified which kinds of plants could furnish, after processing, an indigo-containing liquid. One experimental law specifies the temperature of heating and its duration that yields Maya blue, and another describes the formation of a unique complex of organic molecules bound to the crystal lattice of palygorskite clay. Even the use-related recipe for Chaco cylinder jars embodies generalizations, assuming that the cacao was prepared in, transported by, and consumed from these vessels. Empirical generalizations designated the appropriate vessels and substances for producing the chocolate drink. And experimental laws about utilitarian functions ensured that the vessels had the performance characteristics necessary for interactions in activities.

These case studies may imply to some readers that our models should contain as many behavioral details as possible. For answering some archaeological questions, however, a detailed recipe is unnecessary. Merely by knowing that a cacao drink had been contained in the Chacoan vessels, Crown and Hurst (2009) and others have been able to raise questions about the exchange of this material, which had to have come from the south of Mexico. Yet, approximating as closely as possible the science of past societies calls for detailed recipes, as in the case of Maya blue. Alluding to behavioral chain analysis, I pointed out that fine-grained modeling usually requires, in addition to archaeometric analyses, diverse lines of archaeological evidence. And replication experiments are also very helpful.

Beyond illuminating the recipes of prehistoric technologies, archaeometry offers numerous possibilities for studying the apparatus of early modern and modern science. The availability of portable and nondestructive XRF spectrometers allows us to determine the chemical composition of rare—even unique—apparatus in museums. Recently, at the Conservatoire National des Arts et Métiers in Paris, I stood transfixed before a case holding several meter bars, which were until the mid-twentieth century the world standards of linear measure. Their presumed chemical composition (pure platinum) is well attested in the historical record, but I wondered

whether an XRF analysis would disclose discrepancies. Similarly, the chemical composition of the glass tubes, cylinders, and plates used in eighteenth-century electrical experiments has long puzzled me, a puzzle perhaps easily solved with XRF. Rapid and inexpensive archaeometric analyses would also enable us to identify the chemical composition of proprietary technologies such as the filaments of early vacuum tubes.

As the introduction notes, archaeometric studies are almost mandatory in modern archaeological projects. The reason is simple: they furnish entirely new classes of data on which to build inferences of unsurpassed rigor. In the absence of documentary evidence, archaeologists could speculate grandly for decades about the composition of Maya blue or the contents of Chaco cylinder jars, but only compositional analyses done with modern instrumentation answered the questions. With compositional information and other archaeological evidence in hand, including replications, we can model recipes and flesh out these artifacts' societal contexts. Both Maya blue and Chaco cylinder jars are implicated in fascinating socioreligious processes, investigations of which have been immeasurably enriched by archaeometric analyses.

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Part III

The Apparatus of Modern and Early Modern Science

A handful of historians and others have in recent decades engaged apparatus of early modern and modern science (e.g., Baird 2004; Galison 1997; Gooding 1989, 1990a, 1990b; Gooding, Pinch, and Schaffer 1989; Hankins and Silverman 1995; Rothbart 2007; Shapin and Schaffer 1985). Their questions often have an anthropological or sociological flavor. Galison (1997:2), for one, argued "that laboratory machines can command our attention if they are understood as dense with meaning, not only laden with their direct functions, but also embodying strategies of demonstration, work relationships in the laboratory, and material and symbolic connections to the outside cultures in which these machines have roots." By privileging such phenomena, Galison suggests that he can "get at the material culture of a discipline" (p. 2).

Although archaeologists seldom study the artifacts of modern and early modern science, in these domains we can ask behavioral questions and exploit our conceptual and analytical toolkit to answer them. We could learn, for example, how investigators created scientific knowledge through interacting with apparatus in experiments. To underscore this potential, I present case studies on early electrical technologies that treat the interpretation of singular artifacts and expose the potential offered by research on (1) a project's artifact assemblage and (2) the members of an artifact class.

Life History Narratives and Otto von Guericke's "Electrical Machine"[1]

Trained as an engineer and employed as the mayor of Magdeburg in Prussia, Guericke is best known for inventing an air (vacuum) pump. His interest in doing so was to create a microcosm of the heavenly void—and he did. Nowhere else on Earth

[1] Parts of this section are adapted from Schiffer, Hollenback, and Bell (2003:17–20).

M.B. Schiffer, *The Archaeology of Science*, Manuals in Archaeological Method, Theory and Technique 9, DOI 10.1007/978-3-319-00077-0_7,

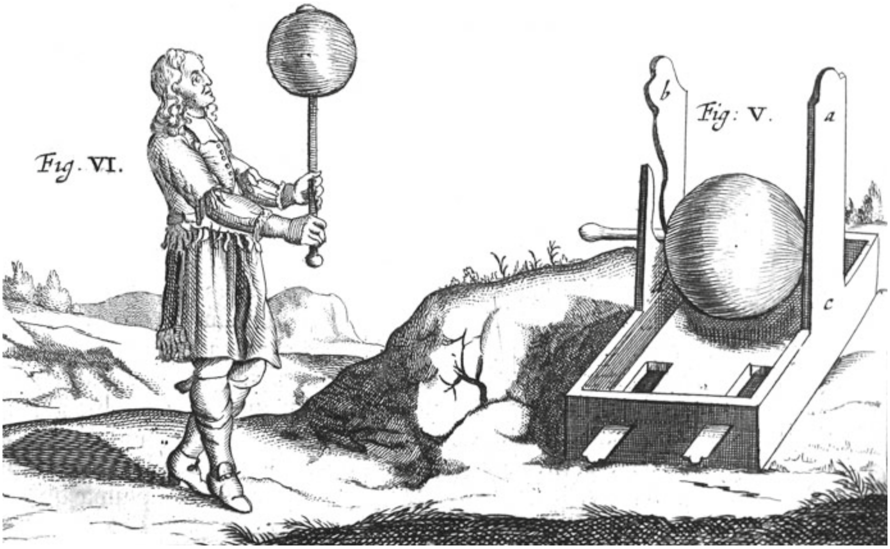

Fig. 1 Otto von Guericke's supposed "Electrical Machine," 1672 (adapted from Guericke (1672) in the Dibner Library, Smithsonian Institution)

at the time did vacua exist save in barometers and in the vessels evacuated by air pumps. Guericke (1994[1672]) reported the air pump experiments in a book that contained his theoretical system of the world, a world filled with occult "virtues" inhering in all matter that exert forces on other physical bodies heavenly and earthly. This treatise also held a thin chapter describing experiments with a small sulfur globe, which he had formed in a glass mold (pp. 227–231). The experiments aimed to reveal the variety of virtues that the globe contained.

After liberating the globe from the mold, Guericke passed a rod through its center and affixed a handle to one end. In this configuration he could place the globe on a wooden stand and perform experiments (Fig. 1). Close to the globe's underside he sprinkled small particles of various materials including leaves, paper, and silver. When Guericke rubbed the bottom of the globe with a dry hand, the sulfur attracted the small particles. After he rotated the globe 180°, the particles still adhered to the globe exhibiting, according to Guericke, the "conserving" virtue he equated with gravity. He also removed the globe from its stand, parading it about by the handle. While rubbing the globe, Guericke heard sounds and, in the dark, observed flickers of light; these effects were caused by the "sound-" and "light-producing" virtues. A feather floating above the rubbed globe illustrated the "expulsive" virtue.

Modern researchers agree that the many effects that Guericke created with his apparatus were electrical (electrostatic), but there the agreement ends. Some

contend that his sulfur globe in its wooden stand was the first electrical machine (e.g., Dibner 1984), but others deny it that status (e.g., Hackmann 1979; Heathcote 1950). Some would allow that it was the first electrical machine only if Guericke had understood he was creating *electrical* phenomena, but the evidence on this point is ambiguous. I suggest that this criterion is too stringent because it requires the investigator to have had an (anachronistic) understanding of what the apparatus was doing. By this criterion many investigators would be denied inventor status. Lee DeForest is properly regarded as the inventor of the triode vacuum tube, the foundation of electronics, but he did not know how it worked.

Another move, perhaps one more congenial to the archaeologist, is to define electrical machines on the basis of formal properties and/or performance characteristics. Then one assesses Guericke's apparatus to see if it conforms to the definition. Generalizing from pre-1760 apparatus that were indisputably electrical machines, we could offer the following definition: an electrical machine has a glass globe or cylinder, mounted on an axle, which an investigator can rotate continuously by means of a mechanical linkage. When rubbed, the rotating vessel acquires a charge that may be drawn off and stored on a prime conductor (a long metal tube) or Leyden jar (a glass or ceramic jar with conductive material on the inside and outside). Guericke's apparatus does not meet this definition because it could not be easily rotated while being rubbed—i.e., the rod has a handle, not a crank. As a skilled mechanic, he would have employed a crank had he intended to spin the ball. We could also fashion a more inclusive definition: an electrical machine produces a charge that the investigator can exploit in experiments. By this definition Guericke did build an electrical machine. I note, however, that the former definition is abstracted from later electrical machines, and is thus anachronistic; the latter definition excluded no means for producing charge, and so is too general, almost vacuous. Other definitions are possible, but the lesson here is that the definitional approach is not definitive because definitions vary among researchers.

There is another way to proceed. I suggest that we ask a series of behavioral questions about the apparatus' life history and, if possible, fashion a narrative from the answers. Some illustrative questions are:

1. Why did the investigator undertake the project and develop the apparatus?
2. What resources were needed for making the apparatus?
3. What does the apparatus' design reveal about its anticipated performance characteristics?
4. What were the apparatus' actual performance characteristics?
5. What kinds of interactions took place during the apparatus' use?
6. In employing the apparatus, what effects did the investigator produce, observe, and record?
7. How did the investigator interpret the effects?
8. Did later investigators take the apparatus as a starting point for developing their own apparatus?
9. Did later apparatus embody similar operating principles?

I now return to Guericke's experiments and present a skeletal narrative inspired by these questions. As noted above—and Heathcote (1950) has emphasized—Guericke conducted these experiments in order to illustrate a series of "virtues" that he believed were present in all physical bodies. Although Guericke could have begun this project with no knowledge of prior experiments, he may have been familiar with William Gilbert's (1958[1600]) work, which showed that a variety of materials when rubbed become "electrics"—i.e., capable of attracting small, lightweight particles. Perhaps Gilbert's generalizations led Guericke to use an electric—sulfur—to illustrate the virtues. Someone in Guericke's social position could have easily obtained the materials for the apparatus, and constructing it placed little demand on his ingenuity and crafting skills.

The apparatus' design—a rod passing through the sulfur ball that rested on a wooden stand—indicates that a critical performance characteristic was the ability to turn the ball on its axis. That the rod was fitted with a handle not a crank shows that Guericke did not expect to rotate the ball rapidly. The handle and design of the stand also allowed the ball to be removed easily, carried around, and returned to the stand. Guericke's description of the experiments indicates that the apparatus' "designed-in" performance characteristics were realized in practice: he was able to turn the ball 180°, remove it, and walk around. Together, sulfur ball, stand, small particles, and Guericke made up the core apparatus whose interactions created various effects, including attraction and repulsion, heat and light, and sound. These effects, judged Guericke, were consistent with the tenets of his theory of virtues.

Francis Hauksbee developed an early electrical machine in the first decade of the eighteenth century, but whether he drew inspiration *directly* from Guericke's prior apparatus is unknown. Urged by Newton to seek an understanding of the light produced at the top of a barometer by the sloshing of mercury (the "mercurial phosphorus"), Hauksbee made several machines. His starting point was Boyle's air pump, the latter a modification of Guericke's invention (Guericke's book was available to men of the Royal Society of London, where both Boyle and Hauksbee worked). Hauksbee's simplest machine consisted of a glass globe in a wooden frame that could be spun rapidly by a mechanism consisting of a crank attached to a large pulley and driving belt (Fig. 2). The Hauksbee machine differed from Guericke's apparatus in materials, parts, and configuration and, by enabling continuous rotation, worked on a different principle. Nonetheless, Guericke's apparatus could have served as one among many resources on which Hauksbee drew in designing his machine.

Narratives prompted by the behavioral questions posed above may have gaps that have to be bridged by inference. Even so, the questions help to orient research and expose gaps and may provoke a search for new evidence. By establishing the basis for a contextualized narrative, this approach obviates the need to debate definitions in the sometimes fruitless quest to learn if an apparatus was the first of a kind.

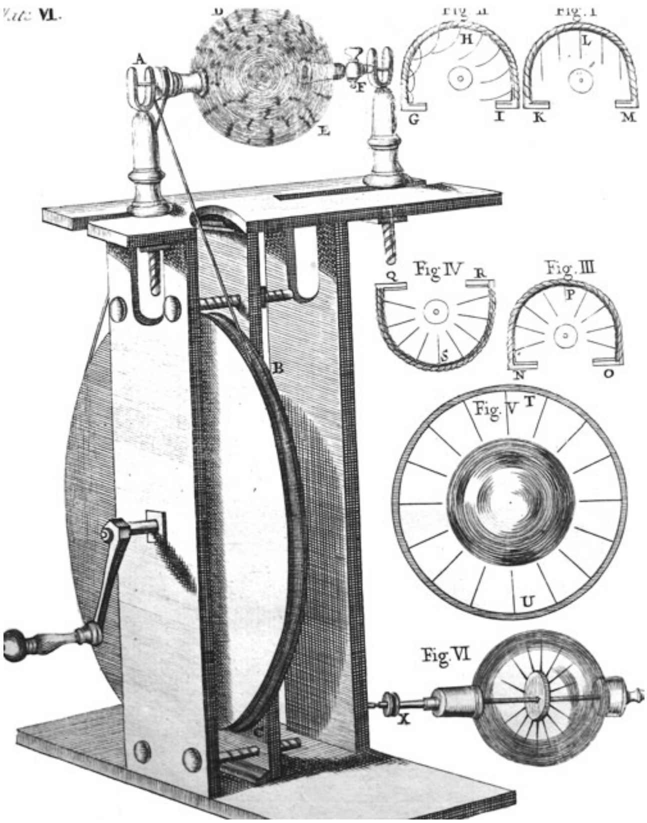

Fig. 2 Francis Hauksbee's electrical machine, ca. 1709 (adapted from Hauksbee and Whiston (1714) in the Dibner Library, Smithsonian Institution)

Cognitive Equivalence? Faraday's "Motor" and Henry's "Teeter-Totter"[2]

Questions from the life history framework enable us to study any apparatus of science, from the simplest to the most complex, which lays the foundation of a narrative. A focus on just one or a few questions may also be instructive, as illustrated by apparatus that figured in early studies of electromagnetism.

In 1820 the Dane, Hans Christian Oersted, caused a stir in the world of Western science by reporting a remarkable effect. When current from a battery is passed through a wire, it causes a compass needle in the wire's immediate vicinity to move. Electricity, therefore, can be transformed into magnetism. Magnetism produced in

[2]Reworked from Schiffer (2008:26–27, 37–38).

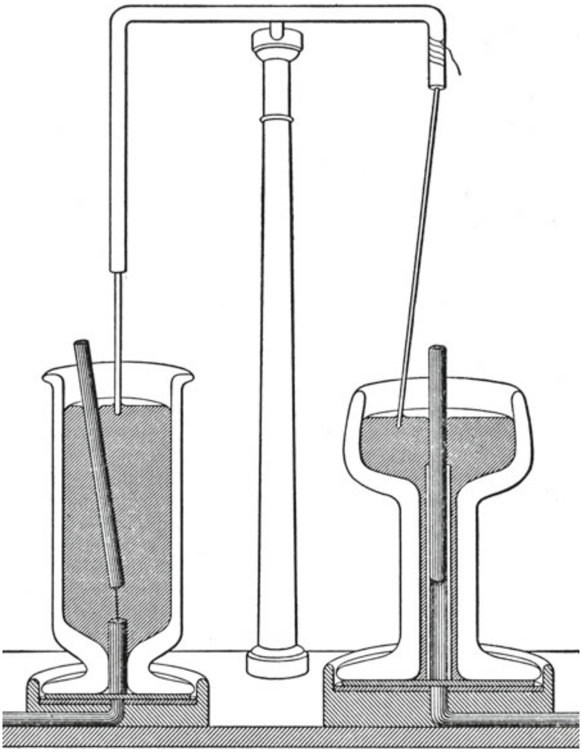

Fig. 3 Two variants of Michael Faraday's rotating device, 1821 (adapted from Faraday (1822), Plate VII)

this way came to be called electromagnetism, and the apparatus came to be called an electromagnet. Oersted's discovery spurred a flurry of projects in several countries. In France Ampère took up the challenge of fashioning basic laws of what he called electrodynamics; in England Michael Faraday, seeking a deeper phenomenological understanding of electromagnetism, began by repeating the experiments of Oersted and Ampère. Moving a compass needle around a current-carrying wire, and noting the needle's direction, Faraday confirmed that the magnetic forces were circular. To shed further light on this effect, he built a small device consisting of a wire crank placed vertically and supported at the top and bottom (Faraday 1822). He then passed a large current through the crank and approached it with a permanent magnet; the crank revolved smartly until it struck the magnet. Had it not been for this impediment, Faraday surmised, the crank would have continued rotating.

Faraday then commissioned several devices with which he showed that a current-carrying wire revolves continuously around a magnetic pole as long as the current flows (Fig. 3). There is no inkling in Faraday's writings that he considered this rotating deviceable to do anything other than exhibit circular magnetic forces. And, to my knowledge, no one in the 1820s called it a motor or engine or prime mover. Decades later, however, some writers on electricity began claiming that Faraday's invention was the *first* electric motor.

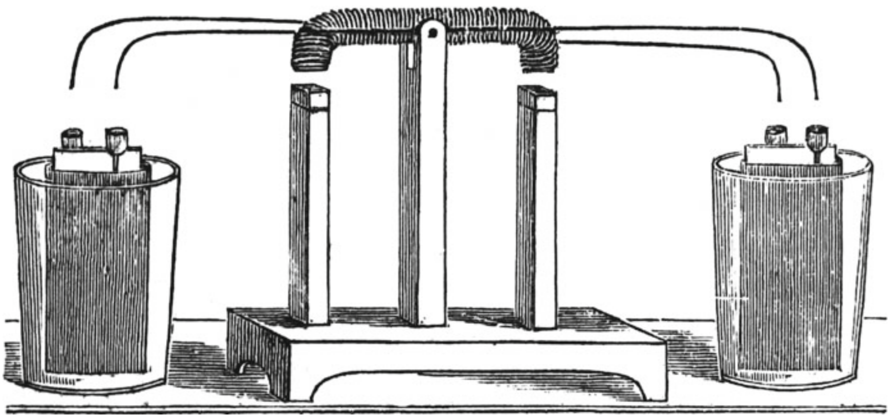

Fig. 4 Joseph Henry's rocking beam motor, 1831 (Henry 1831:342)

An obvious issue, which hinges on definitions, is whether Faraday's apparatus can be regarded as a motor at all. Elsewhere I argued that it was not a motor because it was incapable of doing work in the world (Schiffer 2008:27, 2011:12–13). However, other recent researchers, who apparently defined a motor solely on the basis of an artifact's ability to rotate continuously, did pronounce it a motor (e.g., Bruno 1997:316; Gooding 1990a; Hacking 1983). Given the inconclusiveness of the definitional tack, let us ask a different question, one that can be answered definitively: did Faraday's demonstration device operate by the same experimental law—i.e., have the same knowledge content—as indisputable electric motors that came after 1830? To learn whether there was cognitive continuity between Faraday's device and the motors that followed, let us turn to Joseph Henry's electric teeter-totter.

America's most famous physicist of the mid-nineteenth century and the first Secretary of the Smithsonian Institution, Henry was an early experimenter with electromagnetism. In 1831 he reported an electrical apparatus that produced a rocking motion like a teeter-totter (Henry 1831). Two permanent bar magnets were oriented vertically on a wooden base with north poles at the top (Fig. 4). Between the bar magnets was a stand on which an electromagnet, with wires protruding from each end, could pivot. Each pair of wires supplied current to the electromagnet when dipped into tiny mercury-filled cups connected to a battery. When the electromagnet was energized, its north pole was repelled by one bar magnet while its south pole was drawn to the other. As the electromagnet rocked, it withdrew one pair of wires from the cups and inserted the second pair into the cups on the other side. This immediately reversed the poles of the electromagnet, which then pivoted in the other direction; the rocking motion continued as long as the battery lasted.

Henry regarded his creation as "a philosophical toy" (Henry 1831:340), which meant at the time a device suitable for exhibiting scientific knowledge. It fulfilled that display function exceedingly well, illustrating the generalization that a machine's motion could be sustained when that very motion reversed the

electromagnet's polarity. Working in the 1830s, many inventors, including William Sturgeon in England, Moritz Jacobi in Russia, and Thomas Davenport in the USA, devised mechanical "pole changers"—commutators in modern jargon—and incorporated them into rotary motors, all capable of doing work and based on the operating principle embodied in Henry's electric teeter-totter.

The apparatus of Henry and Faraday both employed electromagnetism to produce continuous motion; both functioned for their creators and contemporaries as devices to exhibit certain electromagnetic effects; and both were unable to drive machines. But there the similarities end because their operating principles differed. In Faraday's device, circular forces around a constantly energized electromagnet caused rotation, whereas Henry's machine depended on alternations of the electromagnet's polarity produced by its own motion. The operating principle of Faraday's rotating apparatus was incorporated by others into the construction of many new display devices, while the operating principle of Henry's teeter-totter made possible (dc) electric motors that actually drove machinery.

It is helpful to look beyond similarities in formal properties and superficial performance characteristics when comparing apparatus, especially those that succeeded each other in time. If our interest is in assessing cognitive continuity—i.e., shared scientific knowledge—between devices, then we should seek an identity in basic operating principles.

Museum Artifacts: Thomas Davenport's Electric Motor[3]

In the mid-1830s a blacksmith from Brandon, Vermont, developed America's first rotary electromagnetic motor that did real work—in his shop. Davenport had encountered Henry's powerful electromagnets, but whether he was inspired by Henry's teeter-totter is a question unanswered in his brief autobiography (Davenport 1851). Nonetheless, there are strong hints of just such a link. Davenport was familiar with the *American Journal of Science and Arts* where Henry described the teeter-totter. In addition to employing the basic operating principle (poles changed by the motor's own motion), which was hardly an obvious move, Davenport's first motors reversed poles by means of wires dipping into and out of small cups of mercury. In any event, Davenport believed his motor sufficiently original to merit a US patent.

Davenport and collaborator Ransom Cook, a carpenter from Saratoga Springs, New York, prepared a patent model. They submitted the model and the paperwork required for it to the Patent Office in Washington, D.C., but their timing was terrible. Before the patent could be issued, a fire broke out in the Patent Office, incinerating their application and the precious model. Davenport and Cook purportedly prepared a *new* model and drawings, which they delivered to the Patent Office in January 1837. On February 25, Davenport received a US Patent (no. 132) for an "electric motor,"

[3] Parts of this section are adapted from Schiffer (2008:70–71).

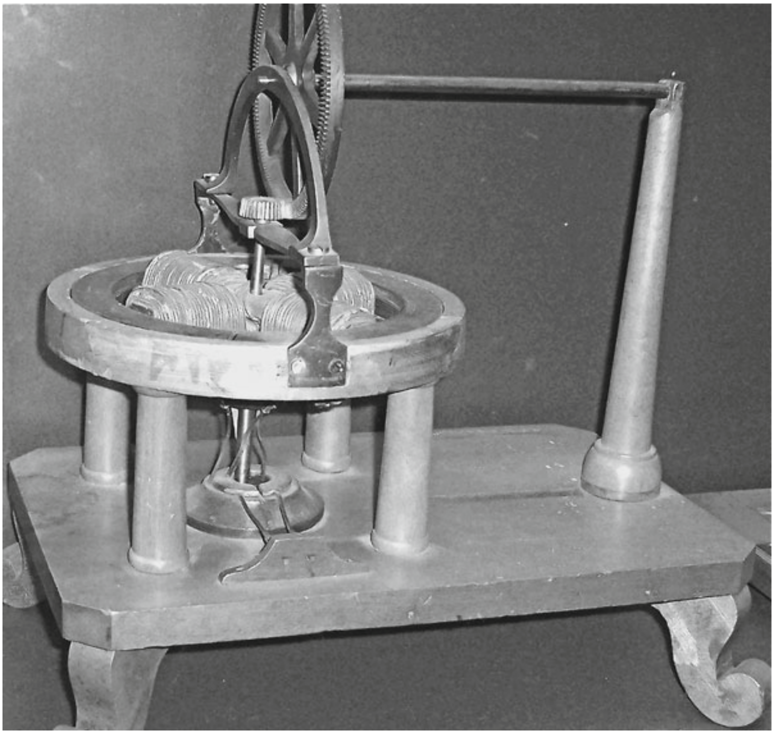

Fig. 5 Patent Model of Thomas Davenport's rotary motor, 1837 or earlier (in the Smithsonian Institution; author's photograph)

the earliest American patent for any electrical thing. Soon the invention was being discussed in scientific and technical journals on both sides of the Atlantic.

The patent model of the Davenport-Cook motor (Fig. 5) has survived and is a national treasure in the Smithsonian Institution, where I examined it in light of questions about its design and post-manufacture history.[4]

An obvious question is why the model has more parts than the patent drawing indicates. Affixed to the vertical rotor is a small gear that engages a much larger gear. The latter in turn drives a horizontal shaft whose other end is supported by a wooden pillar attached to the base. There is also a curious slot in the base, parallel to, and directly below, the shaft. These features would have allowed Davenport to demonstrate the motor's ability to lift an object: one end of a string would be attached to the shaft and threaded through the slot; the other end would suspend the load. Connected to a battery, the motor would have wound the string and lifted the load. This inferred mode of operation, should it be correct, casts doubt on the claim that Davenport and Cook built a *new* motor for submission to the

[4]Smithsonian Institution, National Museum of American History, catalog no. 252,644.

Patent Office. Perhaps it was an older motor that Davenport had been using to entertain paying audiences.

I also infer that the commutator's design would have been unreliable. As part of the commutator, four flat brass wires are soldered to wires emerging from the coils of the electromagnets housed in the circular framework. The wires, descending from the electromagnets, make contact at approximately 90° with two small metal plates mounted on the base. Resembling a washer cut in half with the halves slightly separated, the plates would have been connected to a battery. In this configuration the electromagnets were energized sequentially as the rotary motion brought the wires alternately in contact with each metal plate. However, not only was electrical continuity between the wires and the plates tenuous, but the wires' placement relative to the plates was mechanically weak and could be easily disturbed. As can be seen in Fig. 5, the brass wires are twisted, and thus the motor is inoperable, but we do not know how or when this derangement occurred. Clearly, because of its ineffective design, the commutator would have required frequent tinkering to remain in working order.

I also examined a second Davenport motor, also in the Smithsonian Institution, which ran on a small circular track.[5] This motor has a different commutator, resembling those made by several European investigators, and would have been more reliable. I am tempted to infer that this commutator was an improvement over the one in the patent model, but we don't know when either motor was made.

Science and technology museums bulge with unique, culturally and globally significant apparatus amenable to design analysis—i.e., studying the effects of a recipe's technical choices on an apparatus' performance characteristics (Schiffer 2011). In future studies one could also employ use-alteration analysis (Skibo 2013) to illuminate in more detail actual uses and other post-manufacture interactions. Compositional analysis would permit inferences about the nature and origin of the materials in an apparatus' components. And there is always the option of making a replica and experimenting on it to assess the apparatus' performance characteristics, a move made recently by several historians (e.g., Staubermann 2011).

Project Apparatus as an Artifact Assemblage

As modern science accumulates more and more generalizations, investigators seem to require more complex and costly apparatus for refining existing generalizations and establishing new ones (see "Discovery Processes: Trial Models"). In most sciences the days of plucking the low-hanging fruit with simple and inexpensive equipment passed long ago. Astronomy was probably the first science to take this turn, as even some optical telescopes of the late eighteenth century were massive and

[5] Smithsonian Institution, National Museum of American History, catalog no. 181,825.

expensive. Today, a new telescope capable of probing more deeply into time and space than its predecessors may cost tens of millions of dollars, its construction and operation drawing upon a breathtaking array of organizational, financial, material, and human resources.

Big science now dominates the industrial world as countries, institutions, and large multinational research teams strive to make the next breakthroughs, often competing with their peers. The trend toward gigantism—in apparatus cost, if not in size—has perhaps warped our perceptions of what it took to do cutting-edge projects in the past. Indeed, we may be tempted to regard the much simpler apparatus of earlier centuries as primitive and clumsy, capable of yielding only meager findings. Yet, the stories of earlier science (above) remind us that many discoveries were made with very simple things. The present section underscores that point by turning to additional projects in early electrical science that produced important—indeed, fundamental—experimental laws with apparatus of great simplicity.

Du Fay and the Law of Charges[6]

Charles-François du Fay was a wealthy and well-educated Frenchman who published copiously on many scientific subjects. While he was the director of the king's botanical garden, du Fay became interested in electricity. His most significant finding arose in experiments with the attraction and repulsion of gold leaf. When exposed to a glass tube that had been rubbed (i.e., charged), the gold leaf was first attracted to the tube, then repelled. Intrigued by this effect, du Fay played around with charged specks of gold leaf, presenting them to other electrified substances, including rock crystal, glass, and gum copal. These interactions revealed a surprising effect: a piece of gold leaf repelled by rubbed glass was nonetheless attracted by gum copal and other resinous materials. On the basis of these three-body exercises, many performed on a glass stand, du Fay in 1733 proposed that electricity comes in two varieties. He named them *vitreous*—which arises on rubbed glass—and *resinous*, acquired by resins such as gum copal or amber.

Given two kinds of electricity, du Fay then proposed that objects possessing the same kind of electricity repel each other, whereas those having opposite electricities attract. This experimental law was framed in the most general terms possible, its reach extending far beyond the specific objects that du Fay had manipulated. Although derived from experiments on simple apparatus, this law enabled du Fay and others to explain many puzzling interactions among electrified objects. Rephrased by Benjamin Franklin in terms of positive and negative charges and expressed quantitatively at the end of the century by Charles Augustin Coulomb, du Fay's law remains a cornerstone of modern science.

[6] Adapted from Schiffer, Hollenback, and Bell (2003:30–31).

Alessandro Volta and the Electrochemical Battery[7]

In the early 1790s, Luigi Galvani, a professor of obstetrics at the University of Bologna, published a series of puzzling findings. While experimenting with partially dissected frogs, he placed a metal object against the crural nerve, which caused the frog's legs to twitch. To account for this effect, Galvani offered an elaborate theory that proposed a new kind of electricity—"animal electricity"—that, he claimed, was generated in brains. Other researchers soon replicated his findings, but Galvani's theory did not convince everyone.

Galvani's most persistent critic was Alessandro Volta, a professor of natural philosophy at the University of Pavia. Volta rejected the theory of animal electricity, arguing instead that the muscular movements resulted from an imbalance in electrical fluid caused by external agents such as the metal items wielded by the investigator. Frogs and other animals were simply sensitive electrometers, capable of reacting to minuscule charges.

Volta came to recognize that working with dissected animals was in many senses messy, for creatures complicated an understanding of the underlying physical processes. And so Volta forged new apparatus from common materials. After experimenting with several metals, he came up with the "contact" theory of electricity (actually an experimental law). In an audacious claim that rivaled Galvani's, he suggested that electricity was generated when two different metals or other conductors were placed in contact with one another.

In support of this theory, Volta offered two apparatus: a "crown of cups" and "pile," the first electrochemical batteries. The crown of cups was a line of saltwater-filled cups, usually of glass. They were connected by metal arcs—strips of silver and zinc joined in the middle—whose ends were inserted into the saltwater. A battery of greater force (i.e., tension) could be made by chaining 40 or even 60 cups in a row. The pile, which acquired the moniker "voltaic pile," consisted of a stack of alternating silver and zinc disks, each pair separated by a conducting solution—saltwater-soaked pasteboard. In a tall stack, Volta learned, each additional pair of disks strengthened the pile's ability to shock him.

Volta's demonstration that electricity could be created with such simple apparatus gave experimenters the key to constructing a versatile tool that for the first time generated, without mechanical aids, a continuous flow of current (then called "quantity"). Other investigators tried different combinations of metal electrodes and conductive solutions, creating myriad battery designs. Recall that a battery was indispensible for Oersted's discovery, which gave rise to electromagnetic technologies and the scientific principles that followed from their use. In addition, by providing ample current, the battery helped catapult electrochemistry to the forefront of the sciences. Among that science's first fruits was the discovery of new chemical elements.

[7] Adapted from Schiffer (2008:12–14).

Humphry Davy and the Discovery of Chemical Elements[8]

Named Professor of Chemistry at London's Royal Institution in 1802, the young Humphry Davy conducted research in chemistry and electricity. In experiments reported in 1806, Davy employed compounds of known composition to show that the Institution's large batteries could decompose compounds far more efficiently than could electrical machines. He next applied the technique to potash and soda, common substances that were believed to be compounds. In his first efforts, Davy applied battery current to potash and soda in aqueous solutions, but in both cases only the water decomposed.

Davy surmised that success might come if he excluded water by fusing the potash and soda. He placed a quantity of potash in a platinum spoon and heated it with an alcohol lamp supplied with pure oxygen. A wire from the positive pole of the battery was connected to the spoon, and a wire from the negative pole dipped into the molten potash. The results were dazzling: "a most intense light was exhibited at the negative wire, and a column of flame … arose from the point of contact" (Davy 1808:3). After reversing the polarity, Davy found that tiny globules formed on the spoon, floated to the top of the melt, and burned in the air. The experiment had liberated a previously unknown element: the lightweight and highly reactive metal potassium.

But Davy was not done. He next applied his method to soda and set free sodium. And in a feat of discovery that no individual would ever match, he isolated barium, calcium, strontium, magnesium, and silicon employing various modes of electrical decomposition. The apparatus Davy used was not as simple or cheap to build as du Fay's or Volta's—the Royal Institution's batteries were very large—but batteries having comparable performance characteristics were assembled with the resources available to many investigators. The remaining parts of the apparatus would have been relatively easy to acquire. In the years ahead, electrochemistry became a fount of new generalizations, contributing, for example, to an understanding of chemical bonds, and also led to the development of electrometallurgy (e.g., electroplating)—the first electrical industry (see chapter "Discovery Processes: Trial Models").

Discussion

Study of an apparatus as an artifact assemblage furnishes many research opportunities. This section emphasized one question: did simple apparatus in early electrical science make it possible to fashion fundamental generalizations? The answer was decidedly yes. But we are not limited to asking that question, especially when we engage projects whose apparatus are much more costly and complex. The following are among the questions we might pose, geared to the life history of the project and of its apparatus' constituent artifacts.

[8] Adapted from Schiffer (2008:15–16).

1. What kinds of resources were available to the project's investigator?
2. Did those resource requirements change during the project's existence?
3. When setting forth the project's expected outcome and initial plan, what artifacts did the investigator anticipate requiring?
4. Which artifacts were acquired "off the shelf" from outside manufacturers?
5. Which artifacts were made by project members?
6. Which artifacts were commissioned from outside manufacturers?
7. How was the project organized to acquire, make, and commission its artifacts?
8. How were the artifacts maintained?
9. How was the project organized to maintain its artifacts?
10. During the course of the project, did the artifacts change?
11. If they changed, in what ways did the new ones differ from those originally anticipated?
12. What was the disposition of the artifacts at the end of the project (discarded, reused, curated, etc.)?
13. Did any of the project's artifacts become material resources for other projects?

The answers to these questions can be nested in a narrative that illuminates the contextual factors at work in initiating and sustaining the project.

Functional Differentiation in a Class of Apparatus

Archaeologists are acutely aware that in a long-lived artifact class there is a proliferation of functionally distinct varieties (i.e., "technological differentiation," Schiffer 2002, 2011, chapter 11). This generalization applies as well to the apparatus of science. Indeed, the term "scientific instrument," which is problematic for several reasons (Warner 1990), begs the question by emphasizing just one of several possible functions that members of a given class of apparatus might carry out. Clearly, the archaeology of science includes studies of functional differentiation, which can be illustrated with eighteenth-century electrical machines, an artifact class whose varieties eventually had many functions.

Fortunately, the historian W. D. Hackmann (1978) has written an excellent monograph on early electrical machines, detailing changes in formal properties, performance characteristics, and functions. He also discusses how a particular design was influenced by the investigator's expected outcomes, general and specific. Although Hackmann is not an archaeologist, his work is highly archaeological. We can draw on this work as well as Schiffer, Hollenback, and Bell (2003) to illustrate the potential for research on a class of artifacts or apparatus.

Artifacts commonly have more than one function. Thus, in a class of apparatus, I expect individual artifacts and subclasses to exhibit a mix of utilitarian, symbolic, and emotive functions (on artifact functions, see Schiffer 2011, chapter 2). To study functional differentiation, we situate artifacts in activities, attending to the interactions among the apparatus, investigator, and others over time. Hauksbee's first electrical machines had the utilitarian function of producing an electrical charge that

was available for experiments. But that wasn't all. The Royal Society of London had hired Hauksbee to design and build apparatus for lecturers to use in exhibiting phenomena before an audience. These demonstrations exhibited the Enlightenment tenet that people could obtain new knowledge of the natural world through reason and experiment. Thus, the Hauksbee machines had an important symbolic function: they allowed the audience to witness the lecturer create a variety of stunning visual and acoustic effects—new to science, new to human experience—that entertained while materializing Enlightenment ideology. People in many segments of Western societies gravitated to public lectures to witness the marvelous electrical phenomena firsthand, expecting to be enlightened.

German investigators in the 1740s added accessories to Hauksbee-like machines that made them easier to use. The Hauksbee machine did not allow charge to be stored for later use or transferred easily from the place of its production—the surface of the glass globe—to places where experiments might be more conveniently conducted. To remedy this shortcoming, Georg Bose of Wittenberg University employed a long metal tube that, placed close to the spinning globe, drew off and accumulated the charge; with the machine inactive, the tube was the immediate source of electricity for experiments. In numerous varieties the metal tube became the prime conductor, often fitted out with a metallic comb that harvested charge when held just above the rotating globe. To eliminate the need to rub glass vessels, Johann Winkler, professor at Leipzig, added a mechanical rubber: a pad of leather or linen coated with a mercury amalgam that pressed against the spinning glass and created charge.

By mid-century, lecturers had developed a host of display accessories that were powered by electrical machines, prime conductors, and Leyden jars. Exhibiting many curious visual and acoustic effects, they wowed audiences. These included miniature carillons, cannons that fired a cork, metal foils on glass that spelled out words in sparks, glass tubes that glowed, rotating devices, and fountains that issued a glowing spray of water. In some demonstrations, people used their bodies to convey or store charge.

Some lecturers eschewed machines with rubbers, preferring to remain an intimate part of the apparatus. These people were not merely being "conservative;" rather, they understood that a lecture was a spectacle to which audience members could assign different meanings. I suggest that a lecturer's expectations about symbolic interpretations influenced his choice of electrical machines from among the offerings of instrument makers. Newer machines told a savvy audience that the lecturer was using the latest—"modern"—equipment. At a time when the ideology of progress permeated elite culture, the message of modernity might have resonated with such an audience. And yet, performing before a relatively untutored audience, the lecturer might have wished to trade on his apparent control of occult powers.

Some investigators of electricity, like modern physicists, believed that increasingly powerful machines would create new effects. That is one explanation for the development, later in the eighteenth century, of ever-larger machines, some of which employed glass plates instead of globes or cylinders. Martinus van Marum, as director of the Teyler Museum in Haarlem, the Netherlands, could afford to

commission an enormous electrical machine. It had two rotating glass plates, 65 in. in diameter—the largest that could be cast at that time—a battery of 135 large Leyden jars, and 5 massive brass conductors supported on glass pillars almost 5 ft tall and tipped by huge knobs. Putting the machine in motion required two men to turn the crank, sometimes four men for long experiments.

An apt counterpoint to the image of men wrestling the monster machine was the ornamental carving on its wooden elements. Visually as well as electrically, the machine had been designed to impress and evoke acclaim. By the time of its completion in 1784, the Netherlands was somewhat of a scientific backwater, for in previous decades no luminaries approaching the stature of Christian Huygens and Anton van Leeuwenhoek had appeared in the land of painters and tulips. Van Marum expected that this machine, by allowing him to resolve theoretical disputes on the nature of electricity, would prove that his country was again a hearth of world-class scientific innovation. And so the machine's utilitarian function—it produced sparks 2 ft long and thick as a pen's quill—made possible its symbolic functions. The Teyler behemoth did not resolve the theoretical issues, but it was successfully used in diverse experiments and attracted foreign collaborators. Viewing the machine today, which is still on display in the Teyler Museum, does not fail to elicit surprise and awe.

Wealthy individuals with no philosophic pretensions were also able to commission massive electrical machines with brass and wooden embellishments, functioning mainly to advertise their owners' Enlightenment ideals. And middling people desiring to experiment with electricity and display their command of esoteric knowledge to friends and relatives could choose from relatively inexpensive machines that instrument makers sold in many sizes and varieties. In addition, some investigators commissioned one-off machines for specialized experiments such as studying the effects of electricity on plant growth or determining the best shape for the tip of a lightning conductor. And electromedical practitioners and itinerant lecturers could purchase machines designed for portability. By the end of the century electrical machines exhibited considerable differentiation in design and performance characteristics, enabling them to carry out myriad functions in diverse activities.

Discussion

Archaeologists are keen to explain variation within artifact classes, and have the conceptual and analytical tools to do so (e.g., Schiffer 2011, chapter 11). These tools, I suggest, are equally applicable to the apparatus of science. In general, one begins by defining the apparatus class, which should be based primarily on common operating principles and secondarily on similarities in form. Within the class, the researcher defines subclasses or varieties, fixes them in time and space, and infers their performance characteristics and functions (aware that a given apparatus may have had a mix of functions). Next, the researcher seeks patterns in the differentiation of forms and functions over time. Finally, relevant contextual factors—e.g., changes in activities—are invoked to account for the patterns.

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Thomas Edison's Science

Unlike prehistorians, practitioners of historical, industrial, and contemporary archaeologies have boundless opportunities to study the scientific activities of a named person or organization. In this chapter, I examine the archaeology of Thomas Alva Edison's "invention factories." The earliest was in Menlo Park, New Jersey, where in the course of creating his incandescent lighting system and other technologies the Edison team fashioned many scientific generalizations. The sites of the Menlo Park invention factory and two adjacent structures have been the subject of two archaeological projects, which I draw on for this chapter. Edison built a second invention factory in West Orange, also in New Jersey, which offers great archaeological possibilities because of the marvelous preservation of the earliest structures and some of their original artifact contents. Limited archaeological testing at West Orange has also revealed a potential for studying processes of deposition and disturbance in the laboratory complex. Finally, employing documentary evidence, I delve into Edison's strategy for determining whether a series of revised recipes met the performance requirements of his nickel-iron battery.

Background

Although there is a very long shelf of Edison biographies, I have found Conot (1979) and Israel (1998) to be the most useful. The definitive history of the electric light is Friedel, Israel, and Finn (1986). In addition, the Thomas Edison Papers at Rutgers University has published volumes of Edison papers and has placed online a searchable trove of documents and other resources.[1] In two previous works I have written about some of Edison's activities (Schiffer 2008; Schiffer, Butts, and Grimm 1994).

Thomas Edison was the consummate inventor, and his 1,093 patents remain the US record (Conot 1979:459). Edison cultivated the inventor persona, claiming that "A scientific man busies himself with theory. He is absolutely impractical.

[1] http://edison.rutgers.edu/digital.htm, accessed 21 March 2012.

M.B. Schiffer, *The Archaeology of Science*, Manuals in Archaeological Method, Theory and Technique 9, DOI 10.1007/978-3-319-00077-0_8,

An inventor is essentially practical" (Conot 1979:460). Yet, Edison is known for making several scientific discoveries. The "Edison Effect" was a forerunner of the vacuum tube present in all electronic devices prior to 1950, and Edison's "etheric force" was the kind of electromagnetic radiation that, when rediscovered by others, became the basis of all wireless communication technologies. Edison was not hostile to science much less to theory; rather, he was publicly contemptuous of scientific authorities who doubted, on the basis of theory—not evidence—whether some of his inventions performed as claimed (Schiffer 2008, chapters 21 and 22).

In pursuing technology projects, Edison closely followed *Scientific American*'s prescription for success at invention, which required immersion in a subject's known scientific principles (Schiffer 2008:176–177). Edison's close friend, Henry Ford, put it this way: Edison begins by "making himself completely familiar with the whole fund of knowledge that exists on that subject. He does not aimlessly cut and try. He first of all discovers everything that everyone has done and then repeats all of their experiments to find if they have drawn the correct deductions from them" (Ford and Crowther 2006:31). Edison often learned that available generalizations were inadequate for designing the desired technology, and so he had to undertake subsidiary science projects, as did Ford. Ford put his finger on a general problem that both men faced: "The pioneers in every art … can never obtain the right materials. [Consequently] The electrical industries and the automobile industries have each created a long line of special materials" (Ford and Crowther 2006:4). Indeed, in his celebrated search to identify a suitable filament for the incandescent lamp and in his efforts to improve the positive electrode of the nickel-iron battery, Edison established subsidiary projects to develop materials with the necessary properties.

The Menlo Park Invention Factory

Edison's first inventions were in telegraphy and included duplex and quadruplex systems; he also developed the mimeograph system of reproduction and a stock ticker. Sales of the patents provided him with funds to establish in 1876 an invention factory in the hamlet of Menlo Park, New Jersey, which had clean air and easy rail access to New York City. There he bought land and on it erected a two-storey, multifunctional building containing a machine shop, space for experiments, library, and storage areas for chemicals and other materials. After he outgrew this building, Edison added a machine shop, glass blowers' shed, carpenter's shed, and an office; adjacent to the office was a subterranean vault for document storage.

Edison was not a lone inventor. Indeed, his greatest invention may have been organizational: creating the first industrial R&D laboratory. At Menlo Park he assembled a team of men with varied knowledge and skills, including tinsmith, mathematician, civil engineer, draftsman, machinists, glass blowers, and sundry assistants, who could help to materialize his visions (Gall and Veit 2005:6-43, 6-44). The inventions this team turned out included the phonograph, incandescent lamp, improved telephone transmitter, and an entire electrical system to power lamps and

motors. Other inventors struggled to create an incandescent lamp and lighting system, but none could draw on a stock of human and material resources comparable to those Edison had at his disposal.

Menlo Park as an institution was a signal success, but in 1882 Edison moved his operation to Manhattan, and later abandoned Menlo Park. In the following decades, the vacant buildings were colonized for residences, Sunday school, brass band headquarters, gardens and pigpen, and fire truck storage. The office was destroyed by fire in 1919 (Gall and Veit 2005:6-28, 29), and the remaining buildings fell into disuse and deteriorated.

Henry Ford acquired the Menlo Park properties in 1928 in preparation for creating a permanent tribute to Edison. But instead of restoring the invention factory there, he sent a crew to disassemble the structural remains. And, according to Ford, digging in a midden near the laboratory building yielded 26 barrels of "discarded paraphernalia and remains of experiments" (Ford and Crowther 2006:54). The structural materials, barrels of booty, and even some soil were shipped to Dearborn, Michigan, home to Ford and his factories. With the aged Edison's cooperation, replica structures incorporating some original materials were built at Greenfield Village, where they remain today as a tourist destination, stocked with original and facsimile artifacts.

Ford sold the Menlo Park properties to New Jersey, which created the Edison State Park. In 1937 the modern Edison Memorial Tower was built, replacing an earlier one that had burned down, along with a caretaker's cottage and garage; in the 1960s the latter two were razed (Gall and Veit 2005:6-50). Today the site hosts the Menlo Park Museum, and the Tower is being restored; both Tower and Museum are managed by a nonprofit corporation.

I have tracked down the fate of the West Orange laboratory artifacts that Ford recovered. According to the Henry Ford Museum's Chief Curator of Industry, J. Marc Greuther, this assemblage was once on display in a small building, which has since been demolished. The artifacts were transferred to the basement of another building, where they remain today, uncatalogued and unstudied, perhaps available for future research (Greuther, personal communication, May 2012).

Archaeological Investigations at Menlo Park

Monmouth University's Project

At the request of the Menlo Park Museum, archaeologist Richard Veit, of Monmouth University, held a "pro bono" field school during 2002 (Gall and Veit 2005:1-1). The research goal was to identify "intact archaeological deposits and cultural features … associated with the Edison-Era (i.e. 1876–1882) occupation" (p. 7-1).

During Phase I, test pits (1 ft in diameter) and auger holes were placed in areas believed to be "undisturbed" (p. 7-1) near the footprints of the Charles Dean House (Lot 26) and the Sarah Jordan Boarding House (Lot 25), both of which had housed

many Edison workers. The Jordan Boarding house had been removed to Dearborn; the Dean House escaped the Ford crew and continued to be occupied for many decades, but it eventually burned and was razed.

Limited testing was also carried out adjacent to the Dean House lot. Excavated when feasible in natural levels, the contents of the test units were sifted through quarter-inch mesh screen; the artifacts, excepting "coal, asbestos, brick, and porcelain floor and wall tiles," were catalogued and curated (p. 7-3). Most Phase I test units yielded historic artifacts, both architectural and domestic (summary tables on pp. 8-5, 8-8). Among the domestic artifacts were the remains of glass vessels and ceramics (whiteware, ironstone, porcelain, redware, and stoneware) as well as lamp chimneys and light bulbs; a small sample of faunal remains was also recovered.

In an Extended Phase I, nine units of varying size and shape were excavated in "archaeologically sensitive areas" (p. 7-4); eight units were also dug in the Dean House site, mainly to find traces of the walls. These units, whose contents and stratigraphy were described in detail, yielded an abundance of artifacts and features, including middens, dating mainly to post-Edison times. Yet, in one unit were found two glass insulator fragments "similar to those Edison might have used on his makeshift, 'tree-like' utility poles in Menlo Park" (p. 8-23).

Limited excavations were also carried out in a second area around the footprint of Edison's office and other nonextant structures from later occupations. Of particular interest was a growing sinkhole at the rear of the office. Investigation revealed the intact remains of Edison's subterranean vault, which had been used to store business and invention records, including hundreds of laboratory notebooks. The vault was of course empty, its precious contents relocated, but this find was deemed important enough to merit a publication in *Historical Archaeology* (Gall, Veit, and Savarese 2007). The only intact and in situ structure remaining from the original Menlo Park complex, the vault was described in detail.

Beyond finding and describing the vault, this project made three other contributions. First, Galls' historical research led to a synthesis "on the establishment and growth of the Edison facilities" and on the "history of post-Edison occupations" (pp. 6-28 to 6-29). Second, on the basis of foundation remains, the fieldwork identified the exact locations of the Dean and Jordan houses, and Edison's office. Third, additional research potential was indicated for the house sites, which I strongly underscore. By my estimate, the testing and excavation units together probably sampled no more than 1–2% of the surface. This hints that unsampled areas at either house site might, as the authors contend, contain intact midden, privy, or well deposits from the Edison era that could illuminate the living conditions of the invention factory's employees. Gall and Veit underscore this research potential and recommend full Phase II investigations should future ground disturbances threaten either property.

In areas directly associated with invention factory itself, Gall and Veit (p. 10-2) identified many lots adjacent to the office that "have a high potential to contain … significant archaeological resources." They also recommended a more comprehensive Phase II investigation if the archaeological record is threatened. Obviously, the nature and extent of any extramural deposits unaffected by post-Edison uses of the

site (including the activities of Ford and of later construction) remain to be documented. It would be especially important to find the midden "excavated" by Ford to learn if intact deposits remain. Also warranted is a search for other middens that received discards from the laboratory as well as middens associated with the glass and machine shops. Such deposits might yield materials made in subsidiary science projects as well as prototypes of devices that failed or were no longer useful.

The Project of Hunter Research, Inc.

The Township of Edison, which leases the Menlo Park properties from New Jersey, obtained a grant to manage the state park that includes the invention factory site. To provide input for the planning process, Hunter Research, Inc. was contracted in 2006 to conduct an archaeological investigation (Burrow et al. 2007). The project had several goals: (1) locate remains of the invention factory buildings and associated features, (2) identify the traces of post-Edison activities, namely Ford's expedition and construction of the Tower, (3) identify areas of "archaeological sensitivity" (p. 1-4). And, of course, find Edison-era artifacts.

The project began fieldwork with a geophysical survey, using ground-penetrating radar and a metal detector, which identified "17 subsurface anomalous areas and four subsurface metallic targets" some of which were judged promising (p. 3-1). In ten of these areas, test trenches of varying length were excavated by machine, followed by hand excavations in selected trenches.

Trenches placed in the likely locations of the carpenter and lamp sheds yielded nothing beyond profiles showing soil disturbances, but other trenches contained Edison-era artifacts and architectural remains. In Trench 2 was found one possible brick footing for the machine shop. Trenches 5 and 6, which sought the glass blowers' shed, encountered five brick piers in the shed's probable location (the latter indicated by historical drawings and photographs). These finds, which included three corners of the shed, allowed determination of its approximate dimensions (13 by 30 ft) and orientation. Trench 5 disgorged a few glass insulator fragments having (patent?) dates of 1870; these were most likely associated with Edison's activities. Located in a very large area delineated by the geophysical testing, about midway between the office, laboratory, and glass glowers' shed, Trench 3 produced a curious stone pavement and more than 400 artifacts, the majority from the Edison era. With the assistance of Paul Israel, the foremost authority on Edison's activities, many of these artifacts were identified.

Chapter 5 was devoted to a discussion of the artifact assemblage and was supported by a full inventory (Appendix B) and five multi-artifact images. The assemblage totaled 556 artifacts from all trenches, almost 36% of which were from the Edison era (p. 5-1). Among the ceramic artifacts were crucible fragments containing green or brown residues as well as cupels (presumably used for assaying metals) containing a "glassy greenish/yellow substance" (p. 5-4). Many glass sherds were recovered; most were fragments of commercial tubes that served as raw material for

the glass blowers. Several pieces of glass were likely part of a vacuum pump used for evacuating light bulbs. Because Edison's crew developed a higher-vacuum pump, such fragments could have come from any number of modifications. Only four glass sherds were identified as fragments of Edison incandescent lamps, perhaps early ones at that; one piece retained its (likely) platinum wires. The excavations also recovered a variety of materials including "copper wire, wood, quartz and trimmed pieces of leather" as well as various sherds of domestic ceramics including stoneware and ironware (p. 5-5). The latter items are not surprising since meals were sometimes taken in the laboratory. Also found were several "chalk cylinders or buttons," likely related to telephone experiments (p. 5-4).

Chemical analyses of soil samples identified heavy metals. This is scarcely surprising since the laboratory was stocked with hundreds of chemicals which, after use in experiments, would have been discarded with other refuse.

The report includes a detailed map showing geophysical "survey targets," test trenches, Monmouth University excavation units, modern features, and hypothetical footprints of Edison structures (Fig. 4.1). As noted above, only 10 of 17 geophysical targets were intersected by a trench, and the majority of sampled targets furnished scant information about Edison-era deposits. Although Edison's occupation of the site was short, his activities were varied and material-intensive. I would expect to find more refuse in concentrations or sheet trash in the vicinity of structures; to date, however, there has been little or no testing in these locations.

Burrow et al. (pp. 7-2, 7-3) end their report with a series of research and management recommendations, the latter based mainly on an archaeological sensitivity map (Fig. 7-1). This map should be considered *highly* provisional, subject to modification in light of additional testing. Although the Edison Tower is on the National Register of Historic Places, the remainder of the invention factory is not. Thus, the report recommends preparation of a revised nomination that includes "the entire site of the Invention Factory complex," noting that it is eligible under multiple criteria (p. 7-3).

As to future research, the authors conclude that "the site clearly holds a great deal of potential to provide more information about Edison's activities at Menlo Park. As a historic site, there is an opportunity to include archaeological programming in the longterm plans" (p. 7-3). The testing program's reach into the subsurface, although of limited coverage, furnished tantalizing hints that further testing might be rewarding, and so testing is recommended in several unsampled areas. The report also suggests that an intensive search of laboratory notebooks might make it possible to associate specific artifacts with particular experiments (pp. 5-1, 7-3) or, I suggest, to a *class* of similar experiments. Because of inconsistencies in the locations and number of structures among historic descriptions, maps, and photographs, testing is proposed to find additional architectural traces so that an accurate map can be prepared.

Beyond refining the map, additional fieldwork may reveal unknown aspects of Edison's experiments. Although many artifacts survive today that represent Edison's conspicuous technological successes and failures, there is a sparse record of the actual artifacts and materials that resulted from experiments on subsidiary science

projects, particularly experiments that failed to produce the expected outcome. These kinds of things were likely discarded, and should be present in Menlo Park middens. Did such experiments leave behind the residues on the crucible fragment and cupel shown, respectively, in Figs. 5.2 and 5.3? This question might be answered through compositional analyses. Residue analyses of pottery and glassware should be given a high priority in future projects.

Although the heavy metals present in soil samples (Appendix D) were labeled "contaminants" (p. 7-2), the latter term is potentially misleading. Post-Edison activities may have introduced some contaminants into the site, but Edison used heavy metals in his apparatus (e.g., mercury and lead) and experiments, and so these residues are an integral part of the archaeological record of invention factory science. As Burrow et al. (p. 7-2) recommend, intensive soil sampling should accompany further excavations, not only to safeguard the health of the field crew but also to discern patterns in the spatial distributions of particular chemicals.

Burrow et al. (p. 7-3) suggest that research on this unique site might be moved forward by "seeking grants and/or support from an academic institution." This is a sensible recommendation because appreciable outside funding would be needed to conduct an expanded testing program along with later excavations. I would add that funding should also be sought from the prosperous corporations that directly benefitted from work done at Menlo Park, such as General Electric and the several Baby Bells (offspring of AT&T).

The West Orange Laboratory Complex

Edison established a second invention factory in 1887, this one at West Orange, New Jersey, just west of Newark. A large, three-storey brick building—the Main Laboratory—housed machine shops, library/office, workrooms, and storerooms. In separate structures were the galvanometer (physics) laboratory, chemistry laboratory, chemical storage and pattern shop, and metallurgical laboratory. Later additions included structures for making phonograph records, an office building, two nickel-plating buildings, a copper-plating building, garage, and sundry outbuildings. The five original structures survive and, especially in the Main Laboratory, some contents remain, much as Edison left them when he died in 1931; the later additions have been razed.

A massive vault built in recent times contains an astonishing record of Edison's activities, including about 3,500 laboratory notebooks, records of his companies, personal and business correspondence, patent materials, and so forth; the library holds tens of thousands of books and journals; and throughout the complex are 400,000 sundry artifacts. The process of cataloguing everything is still ongoing, as the amount of material is overwhelming.

The West Orange complex is known today as the Thomas Edison National Historical Park (TENHP), administered by the National Park Service (NPS), and can be visited by the public. In view of the surviving structures and materials (and

federal regulations), it is not surprising that NPS has commissioned a handful of cultural resources reports, including an archaeological assessment.

Of special interest is the two-volume Historical Furnishings Report. Volume 1 (Millard, Hay, and Gassick 1995a) contains descriptions of the buildings and general summaries of each building's occupation by period (1887–1900, 1901–1914, and 1915–1931). There is also a detailed summary by period of each building's uses, furnishings, and personnel, which was compiled from archival materials, oral history, and more than 100 historic photographs, many of which show people at work. Accompanying the descriptions are lively perspective drawings. This volume also chronicles the post-Edison years, 1932–1962, when, as the Edison commercial empire contracted, the family strove to preserve the laboratory complex and its contents as a shrine. Volume 2 contains the historical photographs cited in Volume 1 along with various inventories and price lists for equipment and supplies (Millard, Hay, and Gassick 1995b).

The Historical Furnishings Report was followed by detailed architectural studies of the five original buildings: Main Laboratory (Yocum 1998a), galvanometer building (Yocum 1998b), chemical laboratory (Yocum 1998c), chemical storage and pattern shop (Yocum 1998d), and metallurgical laboratory (Yocum 1998e). These well-illustrated reports discuss the original construction, modifications, and current condition of the buildings and internal features—everything from walls to windows to toilets. They also furnish a use-history keyed to architectural modifications.

From these splendid volumes an archaeologist could reconstruct the footprint of a specific project along with the equipment it employed and the architectural modifications that it required. These are rich data sources for studying the materiality of subsidiary science projects.

There have also been two archaeological projects at the West Orange Laboratory Complex. The reports allegedly include "sensitive" information and are not released to the public—or even to professional archaeologists (Michelle Ortwein, personal communication, May 2012); with considerable persistence I was able to obtain redacted excerpts.

In 1997, NPS archaeologist Jesse Ponz (2002) conducted test excavations, consisting of seven small units, which encountered Edison-era deposits (1887–1931), including an industrial refuse area, coal ash dump, and clay fill. More than 600 artifacts were recovered: construction debris, assorted hardware such as saw blades and nails, laboratory glassware, rocks and minerals, ash and slag, and "domestic artifacts." Among the latter were bivalve shells and ceramic and glass sherds. Also found was a sealed pre-1906 industrial refuse area containing several features.

Recently, Beadenkopf, Davis, and Wieczorek (2011) prepared an "Archeological Overview and Assessment" of the Laboratory Complex and the nearby Edison home, Glenmont. The report includes information on the natural environment, cultural history, and a periodized history of the Laboratory Complex. Previous archaeological research (i.e., the Ponz report) is summarized, and issues of disturbance and preservation are addressed. The very lengthy Chapter 6, "Known and *Potential* Archeological Resources" (emphasis mine), suggests that the project's major goal was to produce a sensitivity assessment or map of buried cultural resources. In view

of Ponz' (2002) highly limited testing and the largely unknown cultural formation processes of the Laboratory Complex, I fail to see how a sensitivity map could possibly be a reliable guide for planning.

This pessimistic conclusion is reinforced by Chapter 7, which includes "Research Questions." Eleven major research questions subsume bulleted subquestions, each one seeking information about depositional and disturbance processes (pp. 207–209). Clearly, more information is needed about, for example, patterns of refuse and chemical disposal from activity areas (including extant and demolished buildings) during and after the Edison era (cf. Hill 2007). I suggest that information on cultural formation processes useful for planning—and research—may be supplied by prospection techniques such as magnetometry, extensive soil sampling, and a highly focused testing program coupled with a search of the Edison archives and ancillary historical materials.

Strategies for Testing Recipes: Edison's Nickel-Iron Battery

Edison and his teams undertook many innovative technology projects at West Orange, including motion pictures, improved phonographs and records, and an alkaline storage battery for electric automobiles. While researching the early electric automobile, I consulted archival materials at West Orange, for I was especially interested in the storage battery (Schiffer, Butts, and Grimm 1994). The storage battery is mentioned in most Edison biographies and is discussed at length in several studies (e.g., Carlson 1988; Schallenberg 1982; Vanderbilt 1971), but I wanted to learn more about the development process; perhaps a behavioral perspective could furnish insights into the creation of a complex recipe. In preparing the present chapter, I revisited my earlier notes, materials copied at West Orange, and publications in order to answer in more detail two main questions: how did Edison learn whether the evolving recipe yielded a battery that met his performance requirements? And did those performance requirements change over time?

Batteries consist of two or more *cells* connected in parallel for greater current or in series for greater voltage. A storage battery, also known as a secondary battery, operates on the basis of reversible electrochemical reactions, and so can be charged and discharged many times. In the early twentieth century, a storage battery's cell consisted of two metal or metal-compound electrodes, one positive the other negative, immersed in a conductive solution known as the electrolyte. Electrical energy was created during discharge by chemical reactions that altered the electrodes; during charging the electrodes reverted to their former chemical state. A cell is usually enclosed in an insulated container having external connectors for the electrodes. In view of the many possible metals, metal compounds and complexes, conductive solutions, and mechanical arrangements that could be employed, storage batteries—then and now—offer vast design possibilities.

The earliest storage batteries had two lead electrodes and a sulfuric acid electrolyte, as do today's car batteries. The lead-acid battery, although powering the first

generation of electric automobiles (ca. 1895–1901), was heavy and high-maintenance, prone to malfunction when mechanically abused, and rapidly lost the ability to hold a charge. In view of these performance problems, which threatened the survival of the nascent electric automobile industry, Edison determined to develop a rugged and lightweight battery employing an alkaline electrolyte. After an extensive literature search he began experiments in 1899. Because existing engineering science furnished insufficient guidance, most of Edison's experiments were subsidiary science projects seeking to confect new recipes for electrode systems and their construction. Also, as Vanderbilt (1971) pointed out, Edison's experiments created new chemical processes to make the battery's ingredients.

In early experiments Edison learned that an electrode system using cobalt seemed promising from an electrical standpoint, but the material was too expensive.[2] Beginning in 1900 he conducted thousands of experiments with cheaper materials, concluding that a nickel-iron battery with a potassium hydroxide electrolyte was workable. After preliminary tests, Edison proclaimed the battery done. He then established the Edison Storage Battery Company in 1901, raised capital by selling stock while retaining a controlling interest, and built a factory in Silver Lake, New Jersey, to crank out cells (Israel 1998:414). He exhibited the battery at the New York Automobile Show and in other venues, feeding a frenzy of publicity with exaggerated claims. The media, always attentive to Edison, provided effusive praise, as in the *Daily Mining Gazette*'*s* story, "Edison Perfects a Storage Battery That Will Revolutionize the World of Power."[3]

The nickel-iron battery was lighter than lead-acid, but it too had performance deficiencies: a cell's voltage was 1.2 versus 2.0 for lead-acid, it took up more space for a given amount of power, the proper water level was critical, and it was more than twice as expensive as a lead-acid equivalent. And then, after limited sales began in mid-1903, customers reported a litany of failures, from the caustic electrolyte eating through the cases' soldered seams to cells that frothed and sometimes "exploded;" and some cells suffered a severe fall-off in capacity. Word of these woes spread, but a humiliated Edison was unable to devise a quick fix, and critics piled on.

In the conventional telling of the battery story, it is said that Edison shut down the factory and laid off its workers, sold no more batteries, and retreated to the laboratory and resumed experiments.[4] Conot (1979:377), for example, wrote that "Battery

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[2] Schallenberg (1982) provides a detailed account of the early experiments and their results. Vanderbilt (1971) discusses how Edison's factories produced chemicals for the nickel-iron battery.

[3] *The Daily Mining Gazette*, 28 February 1901.

[4] Evidence for the shutdown comes from two letters: (1) Edison to a partner in Berlin, Sigmund Bergmann, 9 December 1904: "I have laid off the mfg force at Glen Ridge for a while as the welded cans … developed entirely too many leaks." (2) W. S. Mallory, Vice President of the Edison Storage Battery Company, to W. E. Gilmore, Vice President of the Edison Manufacturing Co., 26 November 1904: "On December 1st … we will close down the Plant at Silver Lake." Both letters on file, TENHP.

production was shut down entirely." Schallenberg (1982:361) even claimed—misleadingly as I show below—that Edison "[bought] back all the bad cells." The story continues that, after tens of thousands of experiments, including a detour using a cobalt electrode, Edison was satisfied that he had solved the problems of the nickel-iron battery, and so around 1909 with the revised recipe he resumed production and sales.

Believing this story to be incomplete, I sought in the laboratory archives evidence on the users and uses of Edison batteries from 1903 to 1909. Finding such materials enabled me to identify Edison's innovative strategy for evaluating the effects of his modified recipes on battery performance. A storage battery's performance requirements were (and are) many and demanding, and may include efficiency (power out as a percentage of power in), the ability to hold a charge for weeks, ease of charging, ease of maintenance, adequate rate of discharge, high energy density, mechanical durability, affordability, the ability to undergo hundreds or even thousands of charge–discharge cycles, and long uselife. Different end users prioritize different combinations of performance characteristics. Thus, for use in electric automobiles, Edison's critical performance requirements were affordability, energy density, ease of maintenance, mechanical durability, uselife, and ease of charging (see Israel 1998:411), but uselife came to have great importance as Edison anticipated direct competition with ever-improving lead-acid batteries.

Meeting these performance requirements required the design and manufacture of specialized machines for making the materials and parts and for assembling cells (Millard 1990:188). Thus, the complete recipe for an Edison nickel-iron cell includes the parts and materials as well as their interactions with machines during their life histories, from the manufacture of materials, to the forming of parts, to final assembly.

To test cells, Edison put a dozen men to work in a large space on the third floor of the Main Laboratory. Although laboratory tests can assess some performance characteristics, Edison understood that the most critical ones, especially those contributing to uselife and durability, would require much time and *real-world* testing. Such tests required the manufacture of improved cells, but where were they made if the battery factory had been shut down? There are several possible answers. The factory may have been closed only briefly, reopened a few months after Edison claimed to have solved the leakage, capacity fall-off, and other problems. Also, Edison had battery-manufacturing capabilities in several locations—including the West Orange complex itself (Israel 1998:419; Millard 1990:187).

In any event, throughout the supposed fallow period, *Edison was still making cells somewhere and selling them*. This I learned from a list of Edison batteries sold to electric vehicle owners between July 1903 and February 1907.[5] Happily, this list also illuminated Edison's testing strategy because it included owners, make and model of vehicle, number of cells, and date of installation; and it was conveniently

[5] Edison Storage Battery Company, 1907. On file, TENHP.

divided between 96 pleasure vehicles and 248 trucks (a battery for an electric vehicle consisted of between 20 and 64 cells). I have summarized the data on cell sales from this list as follows:

	1903	1904	1905	1906	1907
Pleasure vehicles	617	2,723	90	185	0
Trucks	384	3,102	2,222	9,688	130

A striking pattern is the dramatic drop in sales for pleasure vehicles after 1904, while strong sales continued for trucks until the end of the recording period (January 2007). What was Edison up to? Clearly, he was selling a succession of new versions of the battery to a select group of business customers who used them daily, especially in delivery trucks. The Adams Express Company was the largest user, with 151 trucks in New York, Washington, New Haven, and Philadelphia. This company was so satisfied with the early Edison batteries that its President, L.C. Weir, pressed Edison for more, despite the problems, because they were superior to lead-acid batteries in his business. In early 1906, for example, Weir ordered 50 batteries of 62 cells each, an order that was at least partially filled even though Edison complained that he was losing money on such sales.[6] By choosing to sell almost exclusively to businesses after 1904, Edison had incorporated them into the testing process, once acknowledging that "There is a great amount of experimenting going on in trucks."[7] This strategy made sense for several reasons: (1) it was easier to monitor the batteries in central garages than in widely scattered private homes, (2) batteries were being used under rigorous conditions—20–40 miles per day over bad roads, with one or two charges, in all seasons, and (3) long-term costs to battery users could be calculated.

How could Weir and other business users be satisfied with cells that lost capacity, leaked, and "exploded?" The answer is that Edison serviced their batteries. By at least the fall of 1904, Edison's battery inspector, William G. Bee, regularly visited customers, traveling from town to town, checking on the condition of cells, responding to complaints, and skillfully effecting repairs. Defective cells were fixed or replaced at company expense or for a nominal rebuilding charge. More importantly, Bee's reports provided high-quality feedback on cell performance and also on the economy of using the batteries, which Bee determined from company records. From Bee's reports Edison learned that many problems were caused by improper battery installation or slipshod maintenance. Although most malfunctioning cells could be revived, some recalcitrant problems necessitated more experiments and led to new materials and manufacture processes.[8] For example, in early cells the soldered seams of the metal cans leaked, and so Edison resorted to welding the cans, which required new machinery. Throughout this period, then, Edison was efficiently

[6]Weir to Edison, 9 February 1906. On file, TENHP. Edison to Weir, 1 August 1906. On file, TENHP.

[7]Edison to Frank Denton, 13 December 1907. On File, TENHP.

[8]According to H.F. Parshall, a consulting engineer in London, even early versions of the cells could be revived: Parshall to Edison, 15 April 1904. On file, TENHP.

gathering reliable information on cell defects, modifying the recipe, making new production equipment, and selling a succession of improved cells to companies testing them under realistic conditions.

Despite Bee's optimistic reports and mounting pressure from customers and business associates, Edison did not release any of the improved cells for general sales because he judged that their performance characteristics still fell short of his requirements. In particular, a cell's uselife was only a few years, scarcely better than lead-acid. And because nickel-iron cells would cost more than a lead-acid equivalent, Edison recognized that his battery had to excel greatly in uselife and in other performance characteristics most important to commercial and industrial users. Adding to Edison's challenge was the continuing improvement of lead-acid batteries, which meant that he was aiming at a moving target. Fearing that a marginally better battery would simply not be competitive ("commercial" was his term), Edison continued experimenting and monitoring the batteries in use, supported by profits from his prospering phonograph factory and film studio—and a large loan from Henry Ford.

During 1907 and 1908, Edison worked to perfect a radical design for the positive electrode, which would solve the fickle problem of reduced capacity. Hundreds of alternating layers of nickel flake and nickel hydrate were tamped at high pressure into tubes. Another novel feature was the addition to the electrolyte of a small amount of lithium hydroxide. New machines had to be designed and built for making the pure nickel flake and tubes, and for filling the tubes (Fig. 1), but this retooling took much time. In May of 1908, a triumphant Edison wrote to an associate in England that "At last the battery is finished … These cells will solve the problem in every respect, commercially and otherwise."[9]

As the new machines slowly came on line, cell production ramped up, and by July 1909 several hundred were being made weekly to fill the backlog of orders.[10] In 1910, with factory capacity growing smartly, the battery's commercialization was at last formally announced. Having a uselife of at least 4 years (which Edison guaranteed), higher energy density, exceptional mechanical durability, and fairly easy maintenance, the Edison battery began to attract new customers in large numbers, although only a few electric automobile makers bought them. The nickel-iron battery cost 50 % more than a lead-acid equivalent, but in long-term use it was more economical and more reliable.[11] This battery eventually became one of Edison's most lucrative products, finding applications in commercial vehicles, passenger rail cars, railroad switches and signals, submarines, miners' lamps, and so forth (Israel 1998:421). By 1920, thousands of cells were being manufactured daily in 36 varieties whose performance characteristics were tailored to particular uses. Although complex, the cell's basic design was elegant (Fig. 2).

[9] Edison to Parshall, 19 May 1908. On file, TENHP.

[10] Edison Storage Battery Company, Cell Report, July 1909. On file, TENHP.

[11] Letter, Edison to ? (name illegible), 11 June 1910. On file, TENHP. Vanderbilt (1971:220) furnishes a list of the nickel-iron battery's performance characteristics (positive and negative).

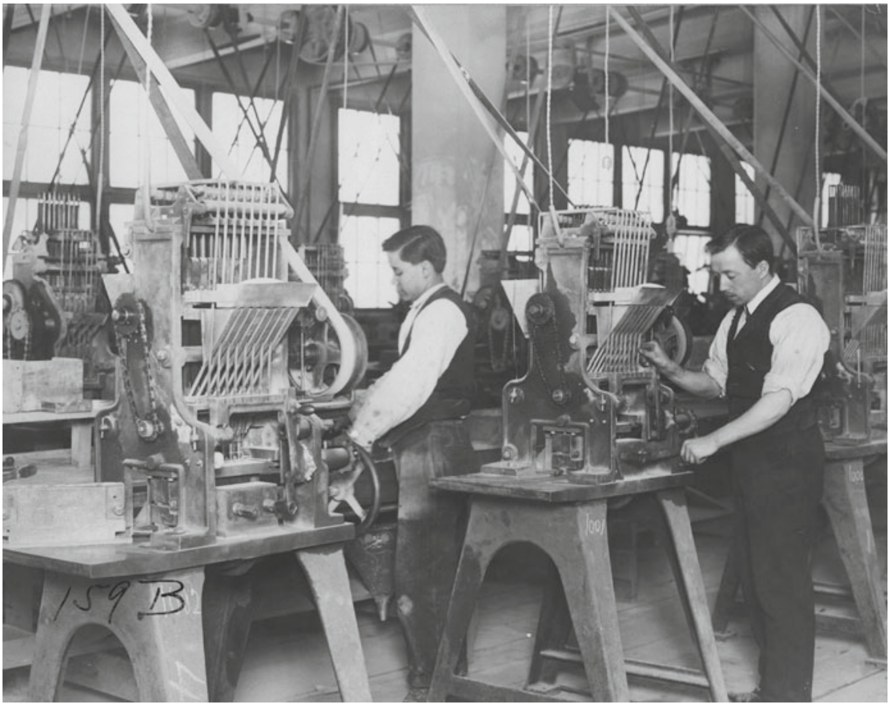

Fig. 1 Edison workers tend machines making positive electrodes for the nickel-iron battery (courtesy of the National Park Service)

Beyond Edison's relentless drive, impressive research-management skills, and 50,000-plus experiments, the battery's success depended heavily on the monitoring of cell performance.[12] This feedback gave Edison confidence that the latest version of the recipe had in fact solved the immediate problem. I doubt that this protracted process was the usual way to evaluate the effectiveness of a product's recipe. Most manufacturers might have cut their losses and turned to other, seemingly more tractable products. But with an optimism bordering on hubris, the expectation that the battery's potential market was large, and the desire to silence his critics, Edison believed that he and his team could eventually redeem the battery. The many years of catering to customers, testing revised recipes under real-world conditions, and comparing his battery to its competitors enabled Edison to judge the most propitious time to bring it to market. But, let me emphasize: what made this lengthy development process possible was Edison's access to capital, for the total cost of this project, including the construction of factories, was in the neighborhood of $2.5 million (Israel 1998:419). In this respect, the nickel-iron battery resembles many ambitious military technology projects, which often proceed despite facing problem after problem because they can depend on continuing support.

[12]Carlson (1988) mentioned the 50,000 experiments and cogently pointed out Edison's research-management skills in connection with the battery project.

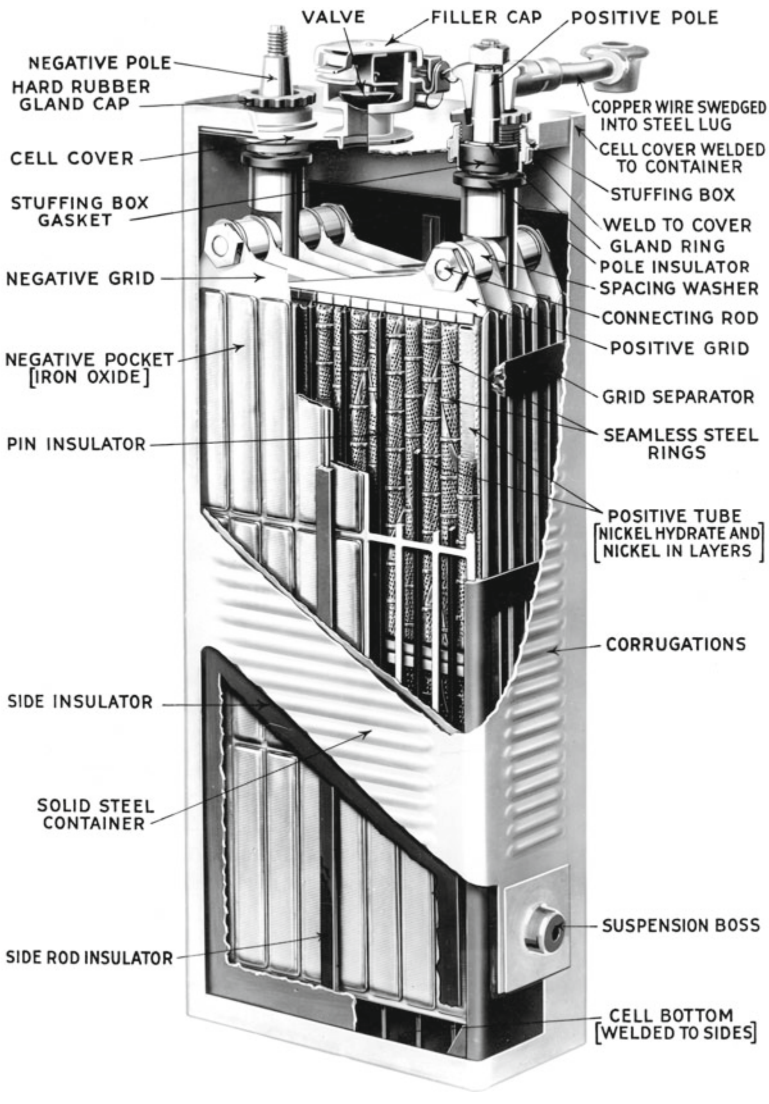

Fig. 2 Edison's nickel-iron battery (courtesy of the National Park Service)

Researchers have mined the Edison archives for decades and crafted many fascinating stories. However, it appears that new insights may come from asking questions about the relationships between people and artifacts in activities aimed at developing recipes. My account of the Edison battery project, first published in 1994 in *Taking Charge*, hinges largely on a list of batteries in actual use, which previous researchers apparently did not consult or judged insignificant. In the future, one could investigate the kinds of strategies employed to

create recipes for complex products in various industrial organizations. Indeed, a comparative study may enable an archaeologist to craft generalizations about development strategies.

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Exploration and Colonization

Beginning with its emergence in Africa, the lineage *Homo* has dramatically expanded the range of inhabited environments. From Africa to Eurasia, to New Guinea and Australia, to the entire New World, and finally to farflung Pacific islands, our species has been the consummate colonizer (Gamble 1994). In modern times, we continue to establish settlements, often in inhospitable places such as Antarctica (see chapter Scientific Expeditions to Antarctica), and to explore heavenly bodies in our solar system (see chapter Archaeology of the Space Age). Not surprisingly, there is a large and growing body of archaeological literature on colonization and exploration (e.g., Capelotti 1999; Fitzhugh and Olin 1993; Riede 2005; Rockman 2009; Rockman and Steele 2003).

Colonization is the establishment of settlements in a place new to the immigrants that may or may not be occupied by other groups. Exploration is a group's first-hand acquisition of knowledge about a place, occupied or unoccupied, sometimes preceding colonization. In modern times, exploration may be done remotely using one-off and expensive technologies such as spacecraft and deep-sea submersibles. Colonization is likely to require activity changes, which in turn may lead to new science and technologies, and exploration by its very nature yields new science.

This chapter has three sections. The first presents scenarios of colonization and exploration, and concludes with a series of questions that may orient research on these processes, paving the way for the case studies. The second section is a case study about the successful Polynesian colonization of New Zealand. However, my inferences about the science that had to be created in making and sustaining this adaptation are somewhat speculative. In the third section I examine England's forays into the Virginia country, a land that then extended far beyond the boundaries of the present-day state of Virginia. The English mounted several expeditions, each of which augmented the quality and quantity of scientific knowledge about this land.

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Scenarios of Exploration and Colonization

The historical and archaeological records indicate that explorers and colonists have found themselves in an immense variety of situations, leading to diverse adaptations—and sometimes a failure to adapt. For present purposes I consider an adaptation to be the entirety of a group's activities, but my emphasis is on the acquisition of material resources for sustaining the group.

In an extreme scenario, the colonists or explorers are closely tethered to a parent community, which supplies resources essential for survival. Modern examples of total dependence include stations in the Arctic and Antarctica as well as spacecraft. In these cases much new science is required, but most is created in advance, especially if people are to venture into hostile environments. Long before a human was launched into space, investigators had to learn how to build craft that could withstand the conditions of launch, space travel, and reentry as well as provide life support for human occupants. An immense amount of new science, from observations to theories, has made possible the creation of technologies and activities to solve these problems.

In a second scenario, colonists and explorers are only loosely coupled, resource-wise, to a parent community, and may bring along technologies without expecting resupply. This situation presents several possibilities, for the new place may be occupied or unoccupied and, compared to the parent community, the environmental resources may be the same or different. These dichotomies are in fact continua. A place may be densely occupied by agricultural communities or seasonally occupied by hunter–gatherers. In the latter case, explorers and colonists may not initially encounter anyone. Likewise, resources may be much different, as in going from a temperate to a tropical environment; somewhat different, as in going from England to North Carolina; or very similar, as in going from one coral atoll to another in Micronesia.

These varied possibilities present different problems of adaptation. If the resources are very similar, then activities may change little, lessening the need for new science and technologies. However, when resources differ greatly, the newcomers have to scramble to devise a viable lifeway. Yet, if the area is already occupied, especially by agriculturalists, there is also the option of obtaining some resources, including science and technologies, from the indigenous inhabitants—perhaps through force or exchange. In addition, hybrid strategies are known from historical accounts. For example, many early colonies in eastern North America depended on periodic visits of supply ships from parent communities and also acquired resources from the local environment and sometimes from indigenous communities.

The season of settlement affects attempts to farm in almost all environments. If the group arrives in an unoccupied temperate environment at the end of summer, for example, it will be too late to plant warm-season crops. In that a case, survival may depend on (1) eating the animals, seeds, and tubers that had been brought along for establishing the subsistence base and (2) turning in great measure to hunting and gathering.

When new technologies and science are required but cannot be appropriated from indigenous communities, colonists are apt to follow several hunting and gathering strategies, perhaps concurrently. The basic strategy is to take advantage of resemblances between old and new resources, applying familiar procurement and processing technologies and recipes to the new species. Optimal foraging theory teaches us that large animals (e.g., ungulates and larger) will be exploited first (if pursuit, capture, and transport times are not extreme) because they are sizable packages of high-quality protein, fat, and other nutrients. People acquainted with hunting large game will employ familiar prey-encounter strategies such as following scats, finding water holes, and locating trails. If available and somewhat familiar, seals, whales, and fish will also be procured. It may also be necessary to exploit, to varying degrees, secondary and tertiary resources such as small animals, nuts, fruits, shellfish, seeds, berries, roots, and insects that come in small packages. These may at first be unfamiliar, but trial and error will identify species acceptable as food. Bitter plant parts are unlikely to be eaten again unless the bitterness is reduced by processing; however, some bitter plants may be retained for their medicinal effects (e.g., emetics and purgatives) and contribute to the group's pharmacopeia. When existing hunting and gathering technologies and recipes prove inadequate, the colonists may adapt or replace them.

The viability of these strategies is affected by mediating factors such as the size and composition of the immigrant population. Large groups are more likely to survive a great loss of members caused by disease, starvation, temperature extremes, violence, and other vicissitudes than small groups, which may die out. However, a very large population has greater resource needs, especially for subsistence, and may be unable to sustain itself solely through hunting and gathering. In small groups, especially, the composition of the colonists in terms of age, sex, and social roles is important because they may lack the entire range of knowledge and skills of the parent community, even when population loss is minimal. In such cases, even if the old and new environments are similar, much new science and technology have to be (re)invented.

Another important mediating factor is the artifacts, cultigens, and domesticated animals that the colonists brought initially. In colonizing eastern North America, British colonists brought along domesticated plants and animals, not all of which thrived in every colony.

Many scholars have speculated about how long it would take colonists, not tethered to outside supply lines or dependent on indigenous groups, to adapt to a new environment. Rockman (2003) suggests that low-frequency phenomena such as extreme annual variation in the availability of biotic resources or in river flow may extend the time for developing an "effective adaptation" to greater than a human generation. Her point is well taken, but before a group can address long-range problems, its members must learn to adapt traditional resources and to procure and process local resources for subsistence, shelters, and clothing (when needed). Unless some kind of adaptation is developed during the first few years, it is unlikely to be developed at all because people will die out, be incorporated into an indigenous group, or return to the parent community. There have been many "lost colonies,"

some of which have been investigated by archaeologists, such as L'Anse aux Meadows, the Norse settlement in Newfoundland (Ingstad and Ingstad 1986), and the 1587 English colony of Roanoke in North Carolina (Noël Hume 1994a).

Groups that piece together an adaptation and survive the initial period of colonization may, as Rockman suggests, encounter extreme environmental events in the future. In response, they may make compensatory behavioral changes: build sturdier shelters after a hurricane, exploit secondary and tertiary wild resources after a crop failure, dig wells after a severe drought, or build levees after a huge flood. Whether these moves create a more stable and effective adaptation over the long term is an empirical question.

Sometimes groups face challenges posed by the unforeseen consequences of their own activities. These may occur on several spatial scales and over the short, medium, and long term. Depletion of faunal resources, exhaustion of firewood, salinization of fields, and soil erosion are common examples. Any combination of natural and anthropogenic environmental problems, which are exacerbated by population growth, may lead to somewhat continuous behavioral change. I suggest that groups with a growing population in a circumscribed territory, such as an island, achieve few stable adaptations. When groups encounter unforeseen problems, regardless of cause, responses are likely to include the creation of new science for enabling behavioral change.

Research Questions

Colonization and exploration processes, which sometimes leave highly obtrusive archaeological remains, generate many descriptions and generalizations. By posing appropriate questions, we can tease out the kinds of scientific knowledge that a group would have created while establishing an adaptation in a new landscape. I offer a sample of such questions below, which are tailored to colonization but may be adapted for exploration. These questions help to structure the case studies.

1. What was the founding population's size and age/sex composition?
2. Was the population large enough to represent the parent community's entire range of scientific knowledge and skills? If not, what might have been missing?
3. What kinds of artifacts, plants, and animals did the founding population bring to their new home?
4. Was the founding population's resource base augmented by interaction with the parent community? If so, in what ways?
5. Did any immigrants arrive after the initial settlement? If so, what did they bring in the way of artifacts, plants and animals, scientific knowledge, and skills?
6. What kinds of traditional scientific knowledge were immediately relevant to developing a subsistence base, erecting shelters, and making clothing?
7. What new activities were undertaken?

8. What new artifacts did the new activities require?
9. What science was developed in the course of those activities?
10. Did some kinds of traditional scientific knowledge prove unhelpful or lead to unsuccessful activities?
11. When traditional activities were no longer performed, was the corresponding science also lost?
12. Did the colonists have to depend, at least in part, on resources furnished—voluntarily or involuntarily—by indigenous communities?
13. Did the colonists establish, at least within the first few years, a viable adaptation? If not, why?
14. What kinds of adaptive problems, if any, were encountered after the initial period of settlement?
15. In response to any such problems, what activities were added, deleted, or modified?
16. What new science was required for the new and modified activities?

These questions, and others that the reader might pose, can help to orient studies of colonization.

The Colonization of New Zealand (*Aotearoa*)

Situated east of Australia in the southwestern Pacific, New Zealand consists of two large islands, North Island and South Island, and many small ones that together comprise about 104,000 square miles—the size of Colorado. Extending almost 1,000 miles north to south, New Zealand possesses vast environmental variation: from subtropical to temperate forests, from volcanic peaks to the glacier-studded Southern Alps. There are also grasslands, swamps, and a coastline of breathtaking length. Distinctive flora and fauna evolved on this long-isolated landmass, including about ten species of moas. These flightless birds, some reaching 3 m tall and weighing 250 kg (Fig. 1), were the main forest herbivores because, with the exception of bats, New Zealand before European colonization lacked terrestrial mammals. Not surprisingly, New Zealand's coastline boasts marine resources, including bony fish, sharks, whales, dolphins, porpoises, seals, and shellfish and other invertebrates.

Since the middle of the twentieth century, archaeology in New Zealand has undergone a florescence, with the major outlines of prehistory well established and tied into the wider prehistory of the Pacific (Kirch and Kahn 2007). Among the syntheses of New Zealand prehistory are Davidson (1984) and Furey and Holdaway (2004); a recent volume focuses on material culture of the Pacific (Anderson, Green, and Leach 2007). In addition, Morrison, Geraghty, and Crowl (1994) edited a four-volume work on the indigenous science of Melanesia, Micronesia, and Polynesia. Anderson (1989) treats the early occupation, the time when moas were exploited. There are also myriad site reports and specialist papers and monographs.

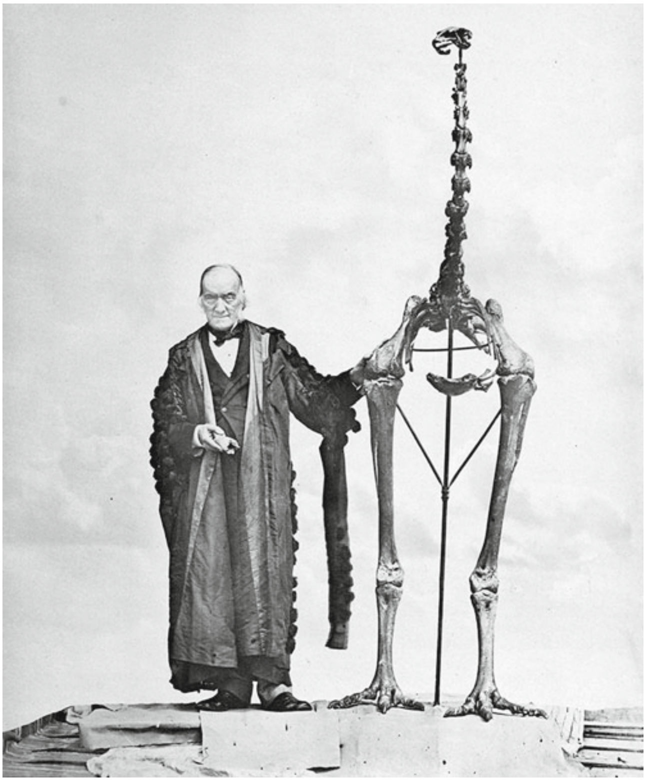

Fig. 1 Richard Owen and a skeleton of the largest moa species (Owen 1879, Plate XCVII)

The indigenous occupation of New Zealand, from the earliest colonists to their descendants—the present-day Māori—is one of biological continuity. The first permanent colonists were Polynesians, who no doubt followed voyages of exploration. Similarities in artifacts, burial practices, and languages have long pointed to Eastern Polynesia, most likely the Society Islands (which includes Tahiti), as the homeland. This inference has been supported recently by genetic and chronological evidence. The analysis of mitochondrial DNA from living peoples indicates that the Eastern Polynesians, including the Māori, are genetically homogeneous and differ from the more heterogeneous Western Polynesians (Penny, Murray-McIntosh, and Harrison 2002).

A comprehensive analysis of radiocarbon dates from Eastern Polynesia has determined when the major island groups were first settled (Wilmshurst et al. 2011). The authors properly focused on the 207 dates from short-lived plants and eggshells of terrestrial birds, the kinds of materials that contain no old carbon (cf. Schiffer 1986). Their analysis shortened most chronologies, indicating that the Society Islands were settled in the twelfth century, followed in the thirteenth century by the Marquesas, Hawaii, Southern Cooks, and Easter (*Rapa Nui*), and New Zealand. The best current date range for New Zealand's colonization is 1230–1280 C.E.

(Wilmshurst et al. 2011:1818), which replicates the findings of an earlier and equally selective analysis of C-14 dates that placed the founding population's arrival at 1250 C.E. or later (Higham and Hogg 1997). This timing coincided with the end of the Medieval Warm Period (ca. 1250 C.E.), after which long-distance voyaging in the Pacific ceased (Nunn et al. 2007; but see Anderson and McFadgen 1990).

In accounting for genetic variation in mitochondrial DNA, simulation modeling suggests that the founding population, which likely arrived on a flotilla of double-hull canoes, included 50–100 women (Penny, Murray-McIntosh, and Harrison 2002). With such a large group, which probably had at least an equal number of men, we may suppose that the founders brought much, if not all, traditional science to New Zealand. There may have been later immigrants, but that has not been established. By assuming 30 generations of 20 years each, growing at a modest rate, the simulation reached the estimated 100,000 Māoris present at the beginning of sustained European contact, ca. 1800 C.E.

The Society Islands are tropical and more environmentally homogeneous than New Zealand. We may assume that the pioneers, who colonized both North and South islands in a matter of decades, encountered different problems of adaptation depending on where they settled; the variation in subsistence and settlement patterns occasioned the creation of much locality-specific science. Villages were established mainly along the coasts, particularly the eastern coasts, which gave ready access to marine resources. Depletion of staple wild resources in part led to adaptive changes, especially after 1500 C.E. (Barber 1996). Throughout prehistory there was, in general, a greater reliance on hunting and gathering in the more temperate South Island.

Owing to the poor preservation of plant macrofossils in New Zealand, inferences about when Polynesian domesticated plants were introduced, the degree of dependence on particular wild and domesticated plants, and changes in plant use remain to be worked out, potentially through reliance on microfossils such as starch grains, pollen, and phytoliths (Horrocks 2004). However, a few well-known patterns are of interest. From the Society Islands the early New Zealanders brought along a full complement of domesticated plants, which included sweet potato, taro, and yam. The pioneers learned that these crops could be grown, but not everywhere and not as easily as in the tropics, and that recipes for planting and harvesting would have to be modified to fit the local environment. And they soon discovered that some of the imported tropical plants—banana, breadfruit, coconut, and sugar cane—could not be grown anywhere; as a result, much science was likely lost. Similarly, many of the useful medicinal and ritual plants of the Society Islands were absent or could not be cultivated. Consequently, trial and error led eventually to a new suite of usable plants, categories to label them, observations and empirical generalizations of their habitats and seasonality, and recipes for processing and use.

Of the Eastern Polynesian domesticated and commensal animals—pigs, dogs, chickens, and rats—only rats and dogs, which were eaten, became established in New Zealand (Clark 1997), but chickens have been found at some sites (Storey, Ladefoged, and Matisoo-Smith 2008). In exploring the inland valleys and lengthy coasts, however, the settlers encountered many new and unfamiliar animals,

particularly moas and fur seals. Not used to human predation, moas and seals could be captured and killed with relative ease; Anderson (1989:151, 157) suggests that the big birds could have been taken with snares and wooden spears. Detailed inspection of moa bones might furnish evidence on hunting technologies. Regardless of hunting technologies, people learned where and when to find the animals, and thus observations and empirical generalizations were created along with new categories; and recipes were required for processing new raw materials and assembling the technologies. Although hearths and traditional Polynesian earth ovens would have sufficed for cooking large animals, new butchery recipes may have been needed. And the people created recipes for fashioning animal bones into ornaments and tools.

For butchery, manufacture of bone artifacts, and other activities, people sought sources of chippable stone, and through trial and error learned which materials could be used for which tasks. The chipped-stone industry led to the creation of material categories, observations of source locations, empirical generalizations about the properties of each material, manufacture recipes, and knowledge about the performance characteristics of finished tools.

New Zealand prior to human occupation was heavily forested, mainly with varied conifers and broadleaf species, including hardwoods, in several major ecozones (McGlone 1989). Although most New Zealand trees were new to the colonists, they could initially approach these resources with general knowledge for harvesting and working wood, which in the Society Islands had been used for constructing dwellings and canoes, and making portable artifacts. However, in New Zealand the settlers still had to learn about the new trees. And so, to their base of botanical knowledge they added tree categories that were coupled to properties and performance characteristics as well as to each type's favored habitats, and recipes for harvesting and working wood were modified as necessary. The growing knowledge of New Zealand woods and other plants enabled the construction of serviceable dwellings and helped to initiate a tradition of ornate carvings for which the Māori are justly famed.

Working of wood employed ground stone artifacts, including chisels and polished stone adzes, and so sources of appropriate materials were sought and identified, including basalt, metamorphosed argillite, and nephrite jade. Because the quarries are highly localized but the finished products widespread, significant exchange likely took place (Leach 1990). After creating suitable recipes, jade and seal ivory were worked into artifacts having important symbolic and emotive functions.

Fishhooks and harpoons made of shell and bone had been employed in Eastern Polynesia, and these technologies were adapted for acquiring bony fishes, sharks, and mammals. Marine invertebrates were also exploited, including cockles and mussels, perhaps with traditional gathering and processing recipes modified.

The early adaptations enabled the ancestral Māori to survive, although analyses of human remains from Wairau Bar, believed to be one of the founding settlements, suggest that the early years had been nutritionally stressful (Buckley et al. 2010). After this initial period, which apparently lasted much less than a generation, the people thrived. However, a heavy reliance on sea mammals and moas was not a sustainable adaptation for a rapidly growing population, especially in a degrading

environment (Nagaoka 2001). According to McGlone (1989:115), "Polynesian settlement of New Zealand … led directly to the extinction or reduction of much of the vertebrate fauna, destruction of half of the lowland and montane forests, and widespread soil erosion." Indeed, a growing human population rapidly decimated the fur seals and drove the moas to extinction. Although the date of the moas' demise remains uncertain, there is general agreement that they were gone within 100–200 years of initial colonization (Holdaway and Jacomb 2000; Nagaoka 2005). As fur seals and moas became scarce, foraging efficiency declined, leading to more intensive processing of moa carcasses and an increasing reliance on second-tier resources (Nagaoka 2001, 2005). And bracken fern root, whose growth was encouraged by fire, became a staple in some regions (Barber 1996).

Eventually, people in North Island came to depend more on the cultivation of sweet potato (*kumara*). As Walter, Smith, and Jacomb (2006:274) remarked, "New Zealand is an unusual case involving a society moving from an agricultural to a predominantly hunting and gathering base and then, following large-scale faunal depletions, back towards agriculture." Even so, many local groups on South Island continued to follow a largely foraging lifeway focused on smaller game such as "fish, small birds and shellfish" (Anderson and Smith 1996:364). Endemic warfare ensued on North Island and in the north of South Island (Davidson 1984), which led to the construction of *pā*, the seemingly ubiquitous fortified hilltop villages (Davidson 1984). Needless to say, the changing adaptations of ancestral Māori required much new science, whose elucidation is beyond the scope of this case study.

The above paragraphs represent a plausible sketch of the kinds of new science that the ancestral Māori likely created as they managed to adapt to unfamiliar environments. Someone closely acquainted with the primary sources of Polynesian archaeology and ethnology would be able to fashion a more complete and nuanced account. Nonetheless, this highly generalized case study indicates the vast research potential that colonization processes offer to the prehistorian interested in the archaeology of science. In addition to New Zealand, promising candidates for such studies are other islands first settled in relatively recent times in the Pacific, Indian Ocean (e.g., Madagascar), and the Caribbean. Once we have in hand a sprinkling of geographically diverse case studies, it should be possible to do comparative analyses that tease out patterns in the creation of science during colonization processes.

Exploring the Virginia Country

England's first effort to establish a colony in the New World was envisioned by its proprietor, Walter Raleigh, as a commercial enterprise in accord with an emerging mercantile model. From the new lands colonists would acquire resources, namely raw materials, which could be sold in England at great profit. In turn, England would sell to the colonists finished goods, also at great profit, and carry on trade with indigenous groups. Thus, identifying resources having commercial potential would be an important focus of the new colony and would require scientific activities.

Quinn (1955:ix) also suggests another rationale for colonization: it would give the English a foothold in the New World from which to attack treasure-laden Spanish galleons traveling from the Caribbean to Spain (see also Kupperman 2009). After all, the English crown sanctioned privateering.

After scouting along the Atlantic seaboard, Raleigh settled on an area that would soon be called "Virginia," whose core was modern Virginia and North Carolina. Named Roanoke, the colony was situated on what is today Roanoke Island in North Carolina. For several reasons Roanoke has attracted much scholarly attention, including that of eminent historical archaeologists J. C. Harrington and Ivor Noël Hume. Not only was Roanoke the first English settlement in the New World, but as a colonial venture it was an utter failure, the fate of its last colonists a mystery. Highly useful histories include Noël Hume (1994a, 1995), Horn (2010), and Quinn (1985); I rely almost exclusively on Quinn's (1985) authoritative book for the historical background.

Agitation to establish English colonies along the Atlantic seaboard of North America accelerated during the 1560s and 1570s, and led to a number of voyages of exploration, including several spearheaded by Raleigh's half brother Humphrey Gilbert. Gilbert, a court favorite as was Raleigh, had obtained in 1578 a patent from Queen Elizabeth that entitled him to establish and rule over colonies. However, Gilbert died at sea in 1583, and the colonization fervor passed to Raleigh, who in 1584 obtained a reissue of Gilbert's patent. The following year Raleigh sent two small vessels to explore the coast in search of a good location for a colony. Although no inventory is known of the ships' contents for this voyage, Quinn (1985:24–27) supplies a plausible reconstruction based on details recorded for an earlier vessel.

In preparation for the voyage, Raleigh hired an Oxford graduate, Thomas Harriot (or Hariot), to instruct his seamen on the instruments for, and mathematics of, navigation at sea. It is not known whether Harriot was present on this expedition, but he would play a large role in the next one. The expedition set off in late April, 1584, and after several stops arrived in the first days of July at the barrier islands of North Carolina (known today as the Outer Banks). On this land still claimed by Spain the explorers ceremoniously took possession in the name of the Queen (Quinn 1985:28–29).

The expedition of 1584 was reported in a narrative by one of its leaders, Arthur Barlowe. He described the flora and fauna on the barrier islands, which were more diverse and abundant than today, and also recounted the group's peaceful interactions with Indians on Roanoke Island. The tribe's subsistence base was a mix of agriculture and hunting and gathering, whose fruits they exchanged with the Englishmen for sundry trinkets. Barlowe's narrative also included geographic observations such as the length of Roanoke Island. After a month's stay, the expedition returned to England with two Indians aboard, Manteo and Wanchese.

With its upbeat descriptions of the land's bountiful resources and friendly, industrious, and generous natives, Barlowe's report "did much to sell the idea that this area was a very Eden" (Quinn 1985:32), merely awaiting the arrival of English settlers. Economic and religious arguments in favor of colonization were also forwarded in a treatise that the cleric Richard Hakluyt wrote at the behest of Raleigh and others. Although Hakluyt's propaganda piece was mainly intended to secure

support from the Queen, she was preoccupied in conflicts with Spain and other continental powers. In the meantime, Manteo and Wanchese were learning English and supplying Raleigh, in whose household they resided, with new information about coastal North Carolina, including its "political geography" (Quinn 1985:43). Despite never having set foot in North America, Raleigh planned a more ambitious expedition.

Among the new expedition's members, Raleigh included Thomas Harriot and John White, the latter a gifted watercolor artist. According to Quinn (1985:49) the two men would be responsible for recording and drawing "everything that would be of interest and importance—the Indian villages, cornfields and gardens, techniques for catching fish, religious edifices and ceremonies, types of individuals and their ranks, together with specimens and drawings of plants, animals, fish, minerals, and … survey the ground in detail and make a general map." In short, Harriot and White were charged with nothing less than producing an illustrated natural history, ethnography, and cartography. The proposed settlement would thus be a base for carrying out scientific activities—clearly in the service of economic goals, as Clucas (2009) has emphasized. In preparation for obtaining useful information in the field, Harriot studied Algonquian with Manteo and Wanchese.

Although the Queen knighted Raleigh and permitted him to name the new country Virginia after her (the "virgin" queen), she furnished only gunpowder and one ship. Hakluyt and a military expert gave Raleigh detailed advice on how to staff and provision the expedition. However, their long lists of diverse occupations, tools, and supplies would have required, in effect, transplanting an entire English town. Raleigh sifted through the lists and arrived at a more spartan inventory.

Raleigh picked a cousin, Richard Grenville, to lead the expedition of seven well-armed vessels carrying an estimated 600 men, of whom half were expected to remain in America (Quinn 1985:56–57). Among the latter were about 150 soldiers, supplied with a full complement of military accessories. The flotilla carried, in addition to "artillery, small arms, gunpowder … a good deal of iron and other metals, in bulk as well as in the form of tools and implements," including agricultural implements (p. 56). "Stores to last the ships six months would be supplemented by dry goods to last the colonists for almost a year … There would also be a good supply of drugs, medicines, and spices" (p. 56). Grains, meat, and fish were also provided as well as iron "spikes and nails, a pitsaw or two, and forge" (p. 57). Curiously, the assemblage apparently lacked fishing gear for the colonists.

The flotilla set sail on April 9, 1585, but did not arrive intact: one small ship was lost, others rendezvoused in the Caribbean where a temporary camp was set up at Guayanilla Bay in Puerto Rico, and a few went directly to the new country. After the flotilla reassembled at the barrier islands, the settlers set to work, rapidly erecting a fort on Roanoke Island to afford protection from any Spanish attack. Inside the enclosure were some dwellings and several additional buildings including a jail and stores. Ralph Lane was the colony's governor.

Exploration proceeded apace, with visits to Roanoke Island, the mainland, and adjacent sounds. Quinn (1985:74) observed that the expedition's "Apothecaries and merchants were discovering commodities that had valuable uses for the English—trees

that produced rich and pleasant gums, fine grapes, drugs … several kinds of flax (one like silk), fine corn … whose cane was thought to make sugar, and clay like the Mediterranean *Terra sigillata*, which had medicinal qualities." Several ships returned to England in August, carrying glowing reports about the Virginia country and leaving behind 108 men to continue exploring.

As for the colonists' skilled occupations, Quinn (1985: 88–90) lists soldier, smith, carpenter, gunsmith, cook, baker, brewer, shoemaker, basketmaker, and thatcher. Also present were miners and a metallurgist named Joachim Gans (also Ganz or Gantz). Gans, a Jew from Prague, had experience in English copper mines, but no historical information has surfaced about his *specific* activities in the colony.

After the last ship departed for England in September, the colony was on its own, left with just two small boats. The settlement was supposed to be self-sufficient, but additional supplies were expected to arrive from England in the spring of 1586, and food stores were supplemented by exchanges with the Indians, which enabled bare survival over late winter and early spring. In view of the colony's many deficiencies—e.g., lack of skilled hunters and fishers, a dearth of agricultural laborers, and the absence of women and children—Quinn (1985:97) remarked that "It seems best to regard the 1585–1586 colony as primarily an experiment in colonization rather than the first step in a carefully thought-out program for establishing a lasting society of English people across the Atlantic." The resupply ship failed to arrive at the appointed time, but salvation came in the form of Francis Drake's formidable fleet, which had just looted and burned St. Augustine, a Spanish settlement in Florida. On June 18 Drake headed to England with the colonists aboard. The settlement on Roanoke Island lay abandoned until Raleigh's next colonization attempt in 1587, whose fascinating history, which includes Virginia Dare, the first English child born in America, and the "lost colony," is beyond the scope of this chapter.

Throughout, John White was busy drawing and painting: the temporary camp in Puerto Rico, plants and animals encountered in the Caribbean, fish that seamen caught during the northward journey, and the many wonders of nature and humankind that the English observed in Virginia country. White's drawings and paintings instantiated many observations and empirical generalizations, and so enriched the store of English scientific knowledge about the New World. His artwork was not included in the first edition of Harriot's slim volume, *A Briefe and True Report of the New Found Land of Virginia*, which appeared in 1588. But the editions of 1590 (e.g., Harriot 1972[1590]), published in Germany in four languages, did showcase a sample of his work rendered in engravings largely faithful to the originals.

Perhaps White's most significant and well-known work is a map of parts of Virginia country explored by land and sea, which he compiled after returning to England from survey sheets made in the field. Quinn (1985:103) reconstructs the surveying techniques and artifacts used by Harriot, White, and helpers, and also points out that "this is the first surveyed map to be made of any part of North America." A distillation of innumerable observations, the map guided navigators for many decades. Today it furnishes a baseline for documenting the many changes that have taken place over the centuries in the dynamic, storm-ravaged barrier islands.

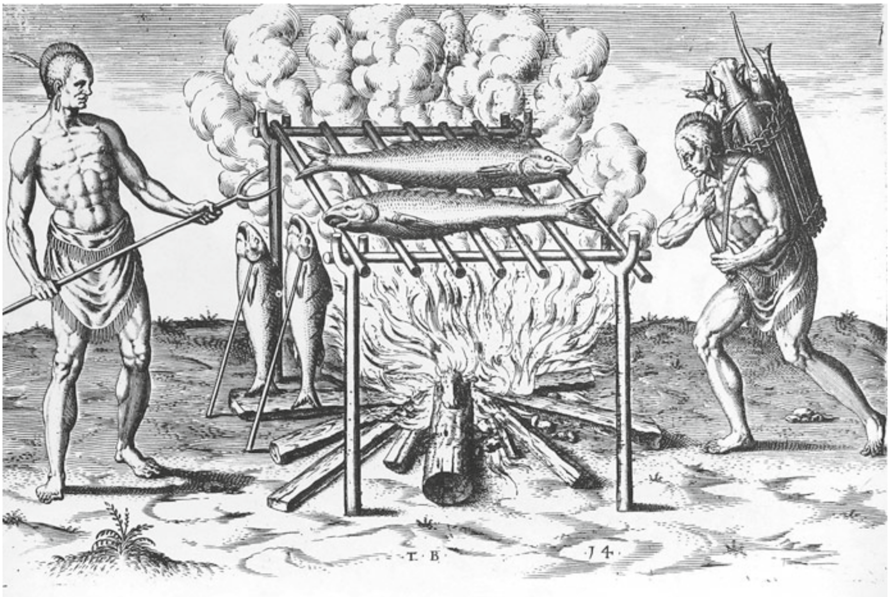

Fig. 2 Indians broiling fish in Virginia Country, 1585–1586 (engraving of a John White painting, from Harriot 1972[1590], courtesy of Dover Press)

A transparent effort to entice new investors and settlers to join Raleigh's colonial enterprise, Harriot's book was published too late to have that effect, for all-out war with Spain ended trans-Altantic travel. As natural history, the book presented only a schematic sample of the expedition's new observations and empirical generalizations, which was predictably skewed toward mineral resources and exploitable flora and fauna—i.e., natural history light. Thus, Harriot mentioned the abundance of familiar plants and animals such as grapes and strawberries, oak and chestnut trees, bears and deer, and also described very briefly, without illustrations, plants and animals new to the English, employing their Indian names. Harriot idealized the Indians as industrious and well-organized people, impressions reinforced by White's stunning paintings of orderly villages and people at work (Fig. 2). Although he did mention that the Indians engaged in warfare (1972:46), he failed to admit that the Englishmen had also become their enemies, having brashly initiated the violence.

Despite its limitations as a natural history of Virginia country, Harriot's book did furnish tantalizing hints about Gans' metallurgical activities. In calling attention to the occurrence of iron, Harriot (1972:10) mentioned that rocks found near the water yielded "iron richly" according to "the triall of a mineral man." Harriot (p. 10) also reported that the local Indians had copper ornaments which upon "triall" were shown to hold silver. Apparently Gans was responsible for assessing in some fashion samples of metals and potential ores. But no mention is made of the apparatus in what must have been a laboratory equipped with a furnace, vessels, and other paraphernalia; this is information that, in the absence of historical documentation, only field archaeology can furnish.

Archaeology of the Roanoke Colony of 1585–1586

The Fort Raleigh National Historic site on Roanoke Island encompasses the area believed to be the location of Raleigh's colonies. Archaeological excavations have taken place there since 1895, and an earthen fortification, presumed to be part of the original 1585 fort, has been reconstructed. Noël Hume, who conducted a major project in the early 1990s, argues convincingly on the basis of stratigraphic and other lines of evidence that the fort was actually a later structure, perhaps eighteenth century (1994a:86–88; 1995:86–105). Apart from undermining received knowledge, Noël Hume's project did unearth convincing remains of the 1585–1586 expedition. In a popular article with the provocative title, "Roanoke Island: America's First Science Center," Noël Hume (1994b) briefly described his project and the traces it recovered of Gans' laboratory (see also Noël Hume 1994a). The detailed preliminary report contains a historical summary, reviews earlier archaeological projects, and presents the methods and results of the new excavations (Noël Hume 1995).

In preparation for his project, Noël Hume reexamined collections obtained by earlier workers. Much to his delight he found science-related vessels: a "Normandy stoneware flask, tin-glazed pharmaceutical [apothecary] pots, and metalworker's crucibles" (Noël Hume 1994a:77). Tellingly, the earlier excavations had also recovered a number of bricks of local manufacture, which "were heavily burned at one end, and several had been ground down so that one side was deeply concave" (1994b, no pagination). None of these materials had been previously linked to Gans' activities, but Noël Hume made the inferential leap because the datable materials were the right age, the vessel forms were consistent with metallurgical activities of that time, and the bricks seemed to have been part of a furnace.

Judging that new excavations might confirm the inference, Noël Hume and his team went to work. The results of the first season, in 1991, seemed unpromising because the site had been badly disturbed by roots, utility trenches, souvenir hunters, modern roads and sidewalks, and previous archaeological trenches. But persistence was rewarded with the discovery, beneath a sandy layer, of a small patch of undisturbed Elizabethan deposits—indeed, a fragment of the occupation surface of Gans' laboratory. Racing against the arrival of a Nor'easter, they recovered in that deposit "the first glass fragments to be found at the site…chips of white flint, sherds of crucible … pieces of charcoal and Indian potsherds," a bar of antimony, and more sherds of Normandy flasks (Noël Hume 1995:72).

As the excavated area was expanded during the 1992 season, additional suggestive artifacts were found, including a pharmaceutical flask, "a lump of copper waste" (p. 78), fragments of roofing tile that might have been used in the furnace, and a piece of lead (pp. 79–85). Re-excavation of a pit dug by J.C. Harrington in 1947 in a different area turned up charcoal fragments that when radiocarbon-dated yielded "an almost ideal bracket of A.D. 1450–1660"(p. 82).[1] This pit, whose function

[1] Noël Hume's description of the date is misleading: the true date has only a 0.67 probability of falling within the bracket (assuming one sigma); he does not mention calibration.

remains unknown, may have been used to make charcoal, perhaps yielding fuel for a furnace. Previous excavators had found in other deposits a variety artifacts that Noël Hume infers were most likely from the laboratory but had been displaced by later disturbances. And he interpreted a semisubterranean, log-walled structure found by Harrington as a shed that may have been part of the laboratory complex (pp. 105–106).

Adding to the weight of evidence in favor of the laboratory inference was an archaeometric analysis of "seven prills from four crucible sherds," a fragment of the antimony bar, and several amorphous copper-containing lumps (Ehrenreich et al. n.d., unpaginated).[2] The samples were analyzed by X-ray photoelectron spectroscopy and X-ray diffraction. The bar of antimony turned out to be largely antimony sulfide, with small amounts of other metals. Citing Agricola's *De Re Metallica* of 1555 and another source, the authors note that "Assayers used antimony sulphide for separating gold from other metals, such as silver, copper, and iron." It is interesting that an "amorphous, copper-base metal lump," which they analyzed, contains more than 5.4 % antimony along with smaller amounts of lead and tin. Perhaps, they suggest, Gans was trying to extract gold from copper artifacts. A second "amorphous, copper-oxide lump" was mainly copper with some lead and iron. This composition is roughly consistent with an "attempt to assay copper ores to determine the richness of the source." The findings of the archaeometric analysis clearly support Harriot's claim and Noël Hume's inference that Gans had carried out metallurgical investigations of copper ores certainly, and Indian artifacts possibly.

Finally, Noël Hume demonstrated that the glass and ceramic sherds of several wares as well as flakes of flint and iron scales present in laboratory-related deposits were consistent with sixteenth-century European metallurgical practice (pp. 117–124). He also argued that the sherds of Indian wares found in these deposits suggest that the vessels had been used in the science center (pp. 124–125). Curiously, in the science-related deposits were recovered more than a dozen sherds of "butter pots" made in the west of England (p. 137). If indeed these pots held butter, it may have had a ritual use. Drawing on *De Re Metallica*, Noël Hume (p. 122, emphasis in original) quotes the caption of a drawing of a man seated near an assayer's furnace and holding a jar: "*The foreman when hungry eats butter*, *that the poison which the crucible exhales may not harm him*, *for this is a special remedy against the poison*."

Thanks to Noël Hume's project, we are confident that some metallurgical analyses took place in the science center, no doubt by Gans, but we lack a detailed reconstruction of these (and any other) activities conducted there. Although the remains of the laboratory are scattered and incomplete, a behavioral chain analysis (Schiffer 1975), which strives to systematically link specific activities to their traces, might help to supply the missing details. In pursuing this strategy of inference, it would be important to involve an archaeometallurgist familiar with the European

[2] This report exists in two versions, neither of which was included in the site report I consulted. Nicholas Luccketti provided me with the preliminary version, Robert M. Ehrenreich the final; I used only information from the latter.

metallurgical texts and archaeological finds in Europe, such as the assemblage from the sixteenth-century alchemist's laboratory at Oberstockstall in Austria (e.g.,. Martinón-Torres 2007; Martinón-Torres and Rehren 2005; Martinón-Torres, Rehren, and von Osten 2003). Perhaps additional residue analyses of the ceramic artifacts, not just of metal traces, would be helpful.

Summing up his project, Noël Hume offered advice to the National Park Service (NPS): "This knowledge not only frees the fort from the indefensible claim that it was built by Ralph Lane as his haven for a hundred and more settlers, but enables interpreters to point to the precise spot where Britain's premier scientist of his day and the first Jewish researcher to work in English-speaking America together assessed the *new found land's* commercial potential" (1995:105, emphasis in original). NPS has embraced Noël Hume's inferences—selectively. The visitor center has an exhibit of some science center artifacts, and this discovery is also mentioned in an NPS brochure (Fort Raleigh 2010). However, the very same brochure states that "Evidence of an Earthen Fort, built by the 1585 or 1587 colonists, and artifacts found nearby are currently our most tangible evidence of the colony."

The Noël Hume excavations not only found indisputable traces of the metallurgical laboratory and undermined the earthen fortification's traditional dating, but also suggested that further work might find additional remains of the 1585–1586 settlement; however, exact locations could not be specified (p. 107). Recent excavations by the First Colony Foundation have turned up more Elizabethan-era artifacts and augmented the evidence supporting the "science center" inference; reports by Nicholas M. Luccketti and his colleagues are available online.[3]

Discussion

In studying colonization and exploration, archaeologists are in an enviable position, for only our fieldwork can yield evidence for inferring the timing of prehistoric colonization processes worldwide. Moreover, in well-studied regions early colonial settlements have been discovered and excavated, parent communities often identified, and strong inferences built about the colonists' subsistence strategies in their new environments. Thus, given the substantial foundation already laid, we can sometimes answer questions about the new science that made possible the colonists' adaptations. We may also examine any exploratory activities that preceded colonization, especially when there is an ample historical record of the expeditions. Eventually, comparative studies will be forthcoming.

The case studies presented above have demonstrated the potential of colonization and exploration studies to contribute to the archaeology of science. In New Zealand, sufficient archaeological research has been accomplished to permit inferences—admittedly very speculative—about the kinds of science that the Polynesians most

[3] http://www.firstcolonyfoundation.org/archaeology/dig_reports.aspx, accessed 19 September 2012.

likely created in the course of forging viable adaptations in an unfamiliar landscape. Even in historical contexts, archaeological fieldwork can furnish evidence of scientific activities unreported in the historical record. Indeed, as the case of Raleigh's first Roanoke colony indicates, only through archaeology was it possible to demonstrate that a metallurgical laboratory had been set up and equipped with apparatus common in European laboratories of that time.

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Scientific Expeditions to Antarctica

Antarctica, including adjacent islands, is a unique setting for the archaeology of science because there were no indigenous inhabitants; much of the continent's sites were created by research and support activities performed during the past several centuries. Significantly, investigations on this continent have made many contributions to our current understandings of the Earth system. The limited amount of professional archaeology accomplished so far indicates that the continent's sites hold information about actual behavior that cannot be gleaned from "books, pictorial collections, diaries and other manuscripts, including ship cargo manifests, and…museum collections of artefacts" (Harrowfield 2004:22). There is also considerable potential for comparative studies. Histories of Antarctic expeditions (Headland 2009) and of Antarctic science (Fogg 1992) furnish background information that may help archaeologists to conceive research projects.

The exploration and scientific study of Antarctica may be divided *informally* into three eras: (1) early historic, ca. 1700–1880, when, in national and private expeditions, explorers, seal hunters, and whalers made landings and occasionally left remains (Zarankin and Senatore 2005); (2) heroic, ca. 1880–1945, involving mainly explorers and adventurers who erected structures and left other traces of their activities, and (3) modern, ca. 1945–present, during which at least 30 countries—many motivated by international competitions—established one or more "permanent" (i.e., not seasonal) research stations along with facilities such as refuges, depots, fuel dumps, field huts, air strips, and specialized equipment (Ricardo Roura, personal communication, 20 January 2012).[1]

Some science was done in the early historic era, such as the U.S. Exploring Expedition of the mid-nineteenth century (Fogg 1992:58–67), but the heroic era is best known for treacherous treks to the South Pole. These expeditions had varied research agendas and destinations, and often included scientists who made and

[1]A list of occupied stations, as of 2009, is in https://www.comnap.aq/facilities, accessed 20 January 2012; see also http://en.wikipedia.org/wiki/Research_stations_in_Antarctica, accessed 20 January 2012.

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published systematic observations on temperature, winds, precipitation, biota, sea currents, and other environmental characteristics. Research in the modern era has been immensely productive, yielding thousands of journal articles and book chapters (Roura 2011:153) that have contributed to meteorology, botany and zoology, oceanography, glaciology, geology, climatology, and seismology (Fogg 1992).

Antarctica also has a growing tourism industry (Tin et al. 2008:9). Recently, Roura (2011) investigated the effects of tourism on abandoned research stations and other sites, mainly of the heroic era, some of which have been designated historically significant by Antarctica's international governing body. Roura's project, which also covered the high Arctic archipelago of Svalbard, aimed to discern the mix of cultural and environmental deterioration processes. He visited sites and documented their condition, compared photographs of the same site taken at different times, and engaged in participant-observation of tourist behavior. On the basis of these lines of evidence he offered recommendations for managing Antarctica's cultural resources. Although Roura's report lacks detailed site descriptions, we can infer from the illustrations and text a great deal about how various expeditions coped with the challenges of an unforgiving environment.

Potential for Regional Studies

Roura's monograph hints that much potential lies in regional studies. In treating Antarctica as one region, locational analyses of its sites using GIS along with maps and Google satellite images may identify factors that influenced decisions about the placement of stations, such as accessibility to the founding country by ship or aircraft, anticipated investigations, expected mix of scientific and military uses, local topography and resources, prevailing weather, tides and currents, national territorial claims, proximity to a country's own stations and those of other countries, and the density of existing stations. Some generalizations are already at hand:

> Ease of access has been an important determinant of station location. As a result, most stations are located on or near the coast, in order to facilitate resupply by ship, and half of them are on the Antarctic Peninsula, the part of the continent that is closest to another continent. Most stations are located on ice-free ground, which makes up less than 0.34 % of the surface area of the Antarctic continent (Tin et al. 2008:8–9).

There is an obvious need to refine and qualify such generalizations, adding variables to explain seeming anomalies such as the handful of stations situated in the continent's interior.

One goal of regional research is to tease out political influences affecting the founding and placement of stations. As Fogg (1992:2) notes, "Antarctic science is different because support on a national scale and therefore involvement with politics has usually been necessary for exploration and investigation to be possible…This was so at the very beginning." The history of settlements furnishes some hints about how politics might be discerned from the material record. The earliest research

stations were established by nearby nations Chile and Argentina. In later decades, political factors such as "Nordic rivalries, U.S. and German conflicts over Antarctic territories during World War II, and Cold War strategic thought" (Broadbent 2009:50), as well as the need to demonstrate scientific and technical competence to the international community, provoked other nations—even those far from Antarctica and without a history of polar research (e.g., India, Bulgaria, and Italy)—to establish stations. How have the latecomers in particular chosen locations in view of their stations' dominant political functions?

Potential for Site-Specific Studies: The Case of East Base

In a review of Antarctic archaeology, Harrowfield (2004) has shown that site-specific studies, including excavation and recording of visible remains, have much research potential despite difficult field conditions. Indeed, excavation in ice and permafrost requires the use of tools unfamiliar to most archaeologists, such as chain saw, ice axe, grinder, and electric percussion hammer; and the recovered artifacts often require conservation to prevent rapid deterioration. Most previous site-specific work, sponsored by governments and international organizations, has furnished information to manage the archaeological resources in the face of environmental deterioration, tourism, souvenir hunting, and strident calls to remove all traces of human activity. Even so, recent fieldwork in a variety of heroic-era sites has documented architecture, unused stores, leisure items, cooking and sleeping technologies, and middens. Clearly, site descriptions, photographs, and artifacts recovered during CRM studies can complement the historical record, providing information on construction details, diet, storage practices, extramural activity areas, and refuse disposal patterns (e.g., Harrowfield 1991; McGowan 1998).

A case in point is America's most significant site of the heroic era: East Base (Antarctica Historic Monument No. 55), built on Stonington Island off the Antarctic Peninsula (Fig. 1). It was one of two research stations established in 1940 by the United States Antarctic Service Expedition under the command of Rear Admiral Robert E. Byrd (Broadbent and Rose 2002). West Base, carried away on sea ice, has not survived, and so East Base—where scientific activities were conducted intermittently until 1975—was the USA's first government-sponsored research station of some permanence in Antarctica. Responding to Germany and Japan's newfound interest in the continent, the expedition's explicit purpose was the "investigation and survey of the natural resources of the land and sea areas of the Antarctic Regions" (Sumner Welles, Acting Secretary of State, quoted in Broadbent and Rose 2002:242) to be performed by aerial survey and mapping. Several buildings and huts were erected, including the "science building," which anchored "biological, geological, glaciological, meteorological, and magnetic studies" (Broadbent and Rose 2002:247), some of which were published. After the expedition departed in 1941,

Fig. 1 East Base, Stonington Island, Marguerite Bay, Antarctica, ca. 2007 (Wikimedia Commons, Geoffrey Boys, Photographer)

East Base was reoccupied by the private Ronne Antarctic Research Expedition in 1947–1948, and later by British expeditions. The latter expeditions reused some original structures but others had fallen into disrepair; some refuse deposits were removed and redeposited on sea ice. And vandalism also took its toll on the archaeological resources, which included aircraft and land vehicles.

In 1991, the U.S. National Science Foundation (NSF) created a management plan for East Base. Under an interagency agreement with NSF, the National Park Service (NPS) undertook a brief survey, documenting the structures and other archaeological remains. Following the management recommendations, in 1992 a team undertook some structure stabilization and environmental remediation, made collections of surface artifacts exposed by melting snow, and created in the restored science building a small museum that displays a sample of the recovered artifacts. The detailed archaeological report contains an inventory of artifacts found in dumps, caches, and scattered refuse, much of which can be associated with the original expedition (Spude and Spude 1993). The variety of artifacts is enormous.

Broadbent and Rose (2002:237–238) have underscored the research potential of sites like East Base: "Archaeology is especially helpful for bringing to light the everyday aspects of expeditions. It shifts the spotlight from planners and leaders to all the men and women who made up the expeditions, their work and leisure, what

they wore, ate, and did. Historical archaeology has a rich, and little realized, potential in Antarctica." To this I might add that such studies may provide evidence on abandonment processes and on the deterioration of artifact and structural materials, as in Robert Blanchette's work on wood-rotting fungi (Stone 2009). Despite publication of the 1991–1992 archaeological work at East Base (Spude and Spude 1993), the research potential of this intriguing data set has yet to be realized.

In general, it might be instructive to compare a station's archaeological artifacts with those listed in an expedition's planning documents and ship cargo manifests (taking into account various formation processes). In addition to disclosing details of daily life, such comparisons might indicate how situational factors caused expedition members to alter even thorough plans. A brief perusal of the East Base artifact inventories suggests that we could learn about daily activities from the things likely to be absent from official inventories, including personal items brought by expedition members. These can be placed into several general categories such as reading materials, games, and personal hygiene. Also, many items brought as part of the expedition, such as medicine and food—with much of the latter left in crates as de facto refuse—furnish details about diet and ailments. By integrating archaeological evidence with archival sources, including photographs, and published scientific reports, we could create a well-rounded account of the Byrd expedition.

Potential for Comparative Studies

Beyond fleshing out the historical record of expeditions, archaeological research offers tantalizing possibilities for comparative studies, such as explaining variability and change in infrastructure technologies and the apparatus of scientific activities (for some generalizations, see Fogg 1992). Given the rigors of the Antarctic environment, what kinds of architectural technologies were developed by different expeditions over time to permit the conduct of activities and the survival of investigators? In answering this question, we could exploit existing archaeological and photographic evidence on materials and construction techniques of huts, laboratories, and other structures. To protect investigators working outside from temperatures that could descend to −89.5°C, technologies such as electrically heated suits and the "thermal" boot were developed (Fogg 1992:149). Focusing on performance characteristics, we might examine which technologies were judged successful and which ones not, and seek examples of technologies improvised under duress. Archaeologists could also examine national differences in technological approaches to addressing survival problems.

Also awaiting study is the development of new apparatus for monitoring environmental conditions, observing and recording flora and fauna, and extracting and preserving various sediment, ice, and water samples. A focus on changes in one class of apparatus, such as coring equipment or trawls for sampling marine organisms, could detail the visions of new knowledge that inspired each apparatus'

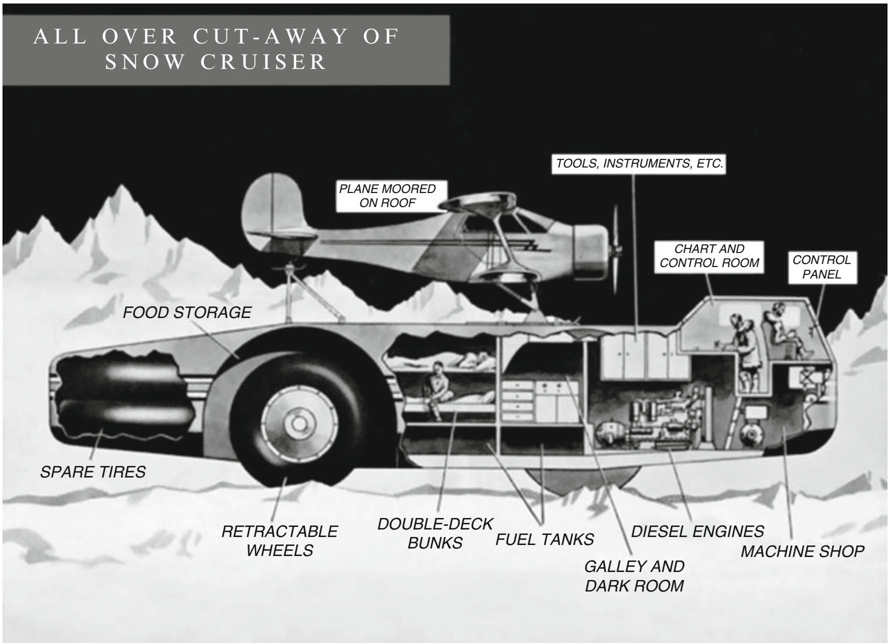

Fig. 2 Poulter's Antarctic snow cruiser (Wikimedia Commons)

development, the role of investigators and funding sources in pursuing development, outcomes of its use, and how the apparatus' deployment may have led to other technological changes.

Another set of questions might focus on how research activities, including placement of stations and other facilities, were affected by changes in transport and communication technologies. What transport technologies made it possible to establish bases deep in the continent's interior, such as the USA's Amundsen-Scott South Pole Station? Although radio technologies allowed an expedition in trouble to summon help, did this seeming security encourage expedition leaders to make riskier overland journeys?

Antarctic expeditions developed technologies for land and sea transport (Fogg 1992:137–141, 149–150). A persistent problem during the heroic era was that of traveling on snow. Dog sleds were reliable but slow and had a limited capacity to move people, supplies, and equipment. Not surprisingly, many expeditions experimented with mechanized land transport. A notorious example is Thomas Poulter's "snow cruiser" (Freitag and Dibbern 1986; Muller 1993). Weighing around 30 tons, it contained an enormous laboratory, living quarters for four people, and a machine shop; it also carried a small plane on its roof and a crane to move it (Fig. 2). Built for the Byrd expedition in 1939 with private funds (ca. $150,000), it was designed to carry all the fuel and supplies needed for surveying the Antarctic interior on its own for an entire year. Its test on sand, which it passed, and arrival at the Boston

wharf where the expedition departed have both been documented on film.[2] After the long sea voyage, the ponderous machine was off-loaded about 4 miles from shore; it crossed the sea ice with little difficulty, but despite having tires 10 ft in diameter and four-wheel drive it made little headway on snow. Almost immediately after arriving, the snow cruiser bogged down for good on sea ice near Byrd's West Base; decades later it and West Base were claimed by the sea.

Poulter's snow cruiser was one of many new or customized technologies designed for overland travel in Antarctica, most of which failed to perform adequately, including a modified World War I tank brought to East Base (Broadbent 2009:55). After World War II, land transportation improved somewhat but deep crevices still swallowed the occasional vehicle. Using the many trials as a diachronic data set, we could assess the performance characteristics contributing to each vehicle's successes and failures and explain each design's origin. It would also be instructive to study what, if anything, later expeditions learned from earlier vehicle trials.

Other Research Opportunities

Ships used for transport, expedition support, or research platforms have occasionally sunk. One of the better-known disasters was the sinking of Otto Nordenskjöld's *Antarctic* in 1903 (Nordenskjöld and Andersson 1977), which occasioned no loss of life but doubtless deposited on the seafloor some expedition-related artifacts. Another loss of an expedition ship and, eventually, much of its apparatus, was Ernest Shackleford's *Endurance* (Alexander 1998). Underwater archaeology in Antarctic waters would require the highest level of diving skill and special equipment but is technically possible. Perhaps the remains of Byrd's West Base and Poulter's snow cruiser could be found.

Another realm of significant research potential lies in ethnoarchaeological studies of occupied research stations. Founded in 1904 and continuously occupied, Argentina's Base Orcadas, located on Laurie Island, is the oldest permanent station in Antarctica still in use. Also, many stations founded in the late 1940s and 1950s remain occupied, such as Chile's Captain Arturo Prat Base on Greenwich Island. In visits supplemented by archival research and excavations in middens, an ethnoarchaeologist could examine how the station's architecture and artifacts have changed during its existence in response to new missions and new activities. In particular, the Chilean and Argentinian bases serve both military and scientific functions. How have these dual roles been negotiated over time and how have they been reflected in artifacts and architecture? Countless questions could also be asked of more recently established stations about, for example, the material correlates of social organization

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[2] Sand test: http://www.youtube.com/watch?v=8zsX6VFraWU, accessed 28 January 2012. Arrival in Boston: http://www.youtube.com/watch?v=h1QtGVVt1Kw&feature=endscreen&NR=1, accessed 28 January 2012.

in a station's highly constrained interior space. We could also study patterns of social interaction and activities in relation to normatively defined roles.

There is no hard-and-fast boundary in Antarctica between exploration and colonization. Stations are colonial installations whose functions include exploration. The continent holds a rich record of these scientific activities that has been little studied. Further archaeological work—regional, site-specific, and comparative—promises to yield insights into diverse subjects such as international competitions, technological change, and the daily lives of "settlers." Creative archaeologists can devise many questions to exploit the continent's unique record of scientific activities.

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The US Nuclear Establishment

In the midst of World War II the USA embarked on a clandestine project to build an atomic bomb. The Manhattan Project created a sprawling research, development, and assembly complex at Los Alamos in northern New Mexico. To supply fissionable materials for the bombs, the federal government also established enormous factories at Oak Ridge, Tennessee, and Hanford, Washington. After the war's end, the US nuclear establishment continued to grow by adding laboratories, manufacturing facilities, and testing grounds in several states.

The facilities built for the Manhattan Project resemble small industrial cities, with hundreds of structures spread over many square miles. Each facility could be treated as a region and would afford an archaeologist a lifetime of research. I focus on Los Alamos National Laboratory and on the Nevada National Security Site (formerly the Nevada Test Site) where assembly, maintenance, and testing of new technologies were carried out; both facilities are today in the Department of Energy. Because the amount of source material is so vast, this case study engages only the Manhattan Project and Project Rover (development of a nuclear-thermal rocket engine).

Despite the secrecy surrounding many projects at Los Alamos and the Nevada National Security Site, historical and archaeological investigations have been ongoing in compliance with federal regulations. Although not solving research problems originating in archaeology *per se*, the CRM projects identify, document, and assess the significance of sites and features, bringing to light architecture, facilities, and artifacts of the once-secret US government activities. Moreover, the reports hint that future archaeological research may provide new insights into nuclear-related scientific projects in these places.

The Manhattan Project

The proliferation of Manhattan Project facilities calls attention to the vast developmental distance that separated the prewar discovery of nuclear fission from the fabrication of functioning weapons. The Manhattan Project is an extreme example of

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how a technology project can generate continuous cascades of subsidiary science and technology projects. Before the outbreak of war in Europe, nuclear physics and chemistry were in a rudimentary state and nuclear engineering just a dream, but by the war's end—with the infusion of at least $1.5 billion in federal dollars and the labor of thousands of investigators—these disciplines had reached an astonishing level of maturity. General information on the Manhattan Project comes from Hoddeson et al. (1993), Hughes (2002), and McKay (1984).

In the late 1930s, physicists in Germany, the United Kingdom, and the USA had forecast that nuclear fission could enable construction of a bomb that, in accord with Einstein's equation ($E = mc^2$), would convert a minute amount of matter into an enormous amount of energy. In principle, when a neutron struck the nucleus of an atom of uranium U^{235}, it would disintegrate into daughter elements. The fission process would also eject additional neutrons, causing other uranium atoms to disintegrate in a continuous "chain reaction" (presuming an initial critical mass of U^{235}). The first self-sustaining chain reaction, which released thermal energy, radioactive elements, and more neutrons, was achieved by Enrico Fermi's team at the University of Chicago in December 1942, under the west grandstand of the Alonzo Stagg football stadium. By using tons of pure graphite as a moderating material (to absorb excess neutrons), they were able to control the reaction. Had their equations been flawed, a part of Chicago might have vanished under a mushroom cloud.

Fearing that Germany was developing an atom bomb (later proved groundless), the USA embarked on the Manhattan Project with help from the United Kingdom. The administrative head of the project was General Leslie Groves, an engineer who had supervised construction of the Pentagon; the scientific director was J. Robert Oppenheimer, a nuclear physicist at the University of California, Berkeley. They decided to develop two bombs: one based on uranium the other on plutonium. Uranium occurs naturally in two isotopes, U^{238} and U^{235}, but only the latter, which makes up but 0.72 % of natural uranium, is fissile. A nuclear bomb requires that the percentage of U^{235} be increased or "enriched" through isotopic separation. This was the daunting task that the Manhattan Project set for Oak Ridge. The second bomb required fissile isotopes of plutonium (Pu^{239} and Pu^{241}). Although the transuranic element plutonium occurs naturally in trace amounts (as Pu^{244}), it was first discovered in experiments at Berkeley in 1940. Hanford's task was to produce fissile plutonium in nuclear reactors (then called "piles"). However, neither Oak Ridge nor Hanford produced appreciable amounts of fissile isotopes until well into 1945. (A brief overview of the architecture at the Hanford site is given by Harvey [2002]).

In the meantime, working with minuscule quantities of enriched uranium and plutonium, Los Alamos investigators determined their chemical and physical properties. This required the design and construction of new apparatus of unprecedented sensitivity to perform measurements that could have been done more easily had sufficient material been available. A host of other research questions faced the hundreds of theorists, chemists, metallurgists, and engineers as they performed calculation after calculation and experiment after experiment on how to achieve critical mass and initiate an uncontrolled chain reaction. For example, initiating a chain reaction at critical mass depended on an infusion of outside neutrons from

polonium, which in turn could not be achieved until polonium's properties were known; this required many subsidiary science projects. In addition, to achieve critical mass in the uranium bomb, two subcritical masses were projected at each other in a "gun," the design of which was entirely novel. However, as knowledge of plutonium's properties grew, it became evident that the gun method would not work. An implosion method was chosen instead, which required an arrangement of shaped charges arrayed around a plutonium sphere or "pit." To refine the design of the implosion detonator, many experiments were carried out with conventional explosives—one blast used 100,000 lb of TNT.

At Los Alamos there is a rich historical record of the Manhattan Project buildings, including photographs and architectural drawings. A recent survey and site evaluation project, whose team contained a historical architect, architect, and consulting engineer (McGehee et al. 2003), reported the survival of 51 Manhattan Project structures, of which 44 were deemed eligible for inclusion in the National Register of Historic Places. (Several original structures had been damaged by the Cerro Grande fire of 2000.) These included storage sheds, laboratory and office buildings, firing chamber, compressor building, laboratories, cloud chamber building, firing pit, shop and dark room, grinding building, and magazines. In these buildings the first atomic bombs were designed, manufactured, and their components tested.

The documentation of each structure includes architectural drawings—plans, sections, and elevations—but much of the detail is illegible on pdfs available from Los Alamos National Laboratory. Assuming one can track down original copies, it should be possible to discern activity areas at the time the drawings were made (but many have dates in the 1980s). In addition to the drawings, the report also includes topographic maps on which the structures have been placed and labeled. Because the year of construction is usually known and modifications are sometimes documented, we could plot the growth of laboratory facilities over time and space, and correlate new construction and alterations with the kinds of scientific generalizations being sought and the locational and performance requirements of the new activities (e.g., proximity to other structures, roads, and environmental features).

Regrettably, historical structures and features have been assessed without appreciable archaeological input. Perhaps survey and excavation projects might yield new information about the Manhattan Project. McGehee and Garcia (1999:65) briefly describe refuse disposal practices, noting that building debris might be tossed over a mesa's edge. Such debris, which could be found easily today, might yield information on, for example, how effectively materials were used on a project that had an almost unlimited budget, and on other kinds of artifacts that might be mingled with the building debris. Also, test pits judiciously placed around structures might discern undocumented patterns of refuse disposal. Tests for radioactivity would obviously precede archaeological fieldwork.

Because nuclear physics and chemistry, isotopic separation, and bomb design were on the frontiers of science in the early 1940s, the Manhattan Project became the incubator of new apparatus and new generalizations, exemplifying the cascade

model (Schiffer 2005)—on steroids. This feature alone invites archaeological scrutiny, for it should be possible to focus on any subsidiary technology project, such as the design of the uranium gun or the plutonium implosion device. We could learn about the kinds of generalizations being sought, each requiring new apparatus and perhaps new facilities for the anticipated experiments. We could also ferret out dead-end paths and still-born technologies that represented decision nodes leading, perhaps, to a change in a subsidiary project's direction. The end result would be a model clarifying the interrelationships among the performance requirements of a subsidiary technology, the development of experimental apparatus and facilities, and the creation of new generalizations.

The Manhattan Project established a pattern of large-scale research and development that would be followed by industrial nations after the war (Hughes 2002). Requiring the collaboration of industrial corporations, university scientists and engineers, and governments, "big science" projects employed a hierarchical organization modeled after large corporations and the military, which integrated people and ostensibly independent institutions. Although having some antecedents, the Manhattan Project was the beginning of the *modern* "military–industrial–academic complex" that now dominates much science and technology development in the USA, United Kingdom, Germany, Japan, and other nations. An archaeologist might ask: How was the Manhattan project's organization, with its many subsidiary projects, reflected in the placement of personnel, activities, and facilities? Did people invent integrative activities that socially mitigated top-down control? If so, what artifacts and places were employed?

The vast source materials available on the Manhattan project, including oral histories, autobiographies, critical histories, and declassified research reports, can be augmented by architectural descriptions and archaeological studies. Together, these lines of evidence would enable us to piece together fascinating stories about the development of science in the context of the twentieth century's most horrifically successful technology project.

The Nevada National Security Site

The first test of a nuclear weapon—a plutonium bomb designed and built at Los Alamos—took place in July 1945 at the Alamogordo Bombing and Gunnery Range (now incorporated into the White Sands Missile Range) in southern New Mexico. Code-named Trinity, this successful test was the first and last at this location. The Trinity site, which included bunkers and numerous instruments scattered around ground zero, has been treated in archaeological and historical reports, including oral history (e.g., Duran and Morgan 1995; Merlan 1997; Slater 1996). Later bomb tests were conducted in lagoons at Bikini and Kwajalein atolls in the Pacific in order to measure the effects of above- and below-water blasts on a variety of American, Japanese, and German warships and on the living animals they contained. An underwater survey and assessment

of the many ships sunk during these tests documented a significant potential for underwater archaeology (Delgado, Lenihan, and Murphy 1991). Some ships are today visited by recreational divers.[1]

These atoll locations involved considerable logistical, security, and weather problems as well as the forced relocation of hundreds of native islanders. Searching for a better site, the Atomic Energy Commission settled on U.S. Air Force property in the desert of southern Nevada. There nearly 1,000 nuclear weapons were detonated and a variety of other nuclear technologies tested (Beck 2002). But no bomb tests have been conducted since 1992.

Archaeologist Colleen M. Beck (2002:65) has underscored some of the Nevada National Security Site's vast research potential: "The archaeological study of nuclear testing provides an opportunity to document a class of historic constructions that, for the most part, are either unique or limited to only a few locations in the world." The archaeological remains derive from atmospheric and underground tests, experiments to assess radiation effects on materials and structures, tests of non-weapon nuclear technologies, and support facilities such as the town of Mercury, which also houses laboratories, an archive, and is still occupied (p. 68).

The abundant underground tests left many traces. The bombs were placed near the ends of horizontal or, more commonly, vertical bore holes. After detonation, a crater might appear on the surface, caused by the ground sinking into the blast chamber. A few features associated with blast sites have been found, such as towers, foundations, and varied electrical cables (Beck 2001, 2002:75; Johnson 2002a).

A horizontal tunnel complex and associated exterior facilities were recently surveyed (Jones, Bullard, and Beck 2006). This particular tunnel complex, deemed eligible for nomination to the National Register of Historic Places, was the site of the Rainier Event in 1957, "the first fully contained underground nuclear explosion in the world" (Jones, Bullard, and Beck 2006:1–2). Illuminating a vast infrastructure, the archaeologists described and photographed 21 structures and 20 features inside and outside the tunnel complex, including railroad tracks, fuel tank, high voltage area, electrical panel, communication trailer, compressor station, generators, and the drill holes (Jones, Bullard, and Beck 2006:Table 2). Because the Rainier Event was the first project of its kind, numerous technologies had to be developed such as a self-sealing tunnel, blast-resistant doors, and new drilling techniques, many of which were intended to prevent radiation leaks (pp. 41–42). The traces of these new technologies appear to be well preserved and ought to reward further archaeological study.

Although sites of atmospheric bomb detonation were cleaned up and some were reused, many structures remain. For example, 157 structures were recorded in one area of Frenchman Flat (Beck 2002:68). Because many of these features are unique, the archaeologists had difficulty describing them. Among the more common features are "metal stanchions embedded in square cement blocks" at ground zeroes; these supported towers to which the bombs were tethered (Beck 2002:69). Also

[1] http://www.bikiniatoll.com/divetour1.html, accessed 6 March 2012.

found are underground bunkers as well as metal towers that held instrumentation (Beck 2001). Beck (2002) notes that the earliest bunkers suffered blast damage, but later ones were more robust. The sequence of bunkers might be studied in detail to reveal the learning process that led to blast-resistant construction. Additional features are associated with specific research projects, such as those examining the effects of blasts on different materials and structures at varying distances from ground zeroes (Johnson 2002b).

Perhaps the most curious remains are those of the "Japanese Village," a cluster of Japanese-style houses, fitted with dosimeters, which measured the radiation-shielding effects of structures. The project aimed to provide Japanese biomedical researchers with information about the likely long-term effects of radiation exposure from the Hiroshima and Nagasaki blasts. The wood-frame skeletons of several structures have survived and were recorded (Johnson 2002b). After atmospheric tests were banned in 1963, controlled testing of radiation effects on various materials were performed at the Super Kukla facility, a specialized nuclear reactor whose remains have been documented historically and archaeologically (Drollinger, Goldenberg, and Beck 2000a).

Project Rover: A Nuclear-Thermal Rocket Engine

Although the structures, facilities, and other artifacts at the Nevada National Security Site represent many intriguing projects, I focus on Project Rover, which created several generations of nuclear-thermal engines. My oversimplified descriptions of this project are based mainly on Dewar (2004), Finseth (1991), Fishbine et al. (2011), Sandoval (1997), and Spence (1968). Dewar (2004) engages both the technological challenges and, in excruciating detail, the political context.

Carried out from 1955 to 1973 at a cost of $1.45 billion (Dewar 2004:319), when the USA and the Soviet Union were competing for preeminence in rocketry and space-exploration technologies, Project Rover's expected outcome was a nuclear-thermal engine capable of propelling a rocket. Calculations had shown that such an engine could achieve a far greater power density than one fueled by chemical reactions, and so might cut travel time or permit a heavier payload. And unlike the solid-fuel chemical engines of that time, a nuclear-thermal engine could in principle be started, stopped, and restarted. Because the engine would spew radioactive gas, however, it could not serve as a rocket's first stage.

Throughout its existence Project Rover was about research and development, with no firm applications agreed on by federal agencies, Congress, and Presidents from Eisenhower through Nixon. Some proponents envisioned it as the third stage of a Saturn V rocket; others suggested that it could be used in a spacecraft for ferrying people and supplies between an earth-orbiting space station and a lunar base (Dewar 2004, chapter 13). None of these proposed missions led to a consensus on the part decision-makers. With no designated missions, budgetary constraints at the height of the Vietnam War eventually led to the project's termination despite its technological and scientific successes.

The basic operating principles of a nuclear-thermal engine, already envisioned during the late 1940s (Bussard and DeLauer 1958:1–3), are straightforward. Because a nuclear reactor's core generates a massive amount of heat, it can raise the temperature of a gas flowing through it to more than 2,000°C, preferably a gas of low molecular weight. When liquid hydrogen, which has the lowest molecular weight of any element, is pumped through a reactor's hot fuel elements, it vaporizes immediately, expands enormously, and escapes through a nozzle, thus yielding thrust. The flow of hydrogen also performs the essential function of cooling the core. Early tests were carried out not on complete engines but on reactor cores, as investigators grappled with many design problems.

Although a nuclear engine's basic operating principles are simple, embodying them in functioning hardware was devilishly complex. Like the Manhattan Project, Project Rover spawned a vast array of subsidiary projects that created new apparatus and new generalizations. Moreover, reactor and engine tests necessitated new kinds of massive structures. Accordingly, during the life of Project Rover at least five large building complexes and other facilities were constructed at the Nevada National Security Site dispersed over an area of about 30 square miles. Structures in these complexes were modified as testing activities changed, and new ones were built.

Project Rover passed through several stages corresponding roughly to families of reactors and engines (Finseth 1991). The first stages, ca. 1959–1964, were Kiwi A and Kiwi B, in a reference to New Zealand's flightless bird of that name, for these reactors were intended to be earth-bound, used only for evaluating and tweaking designs. By the end of Kiwi B, the nuclear-thermal design had achieved considerable maturity, although not all performance requirements had been fully met. The next stage, Phoebus, lasted from about 1965–1968, and led to a generation of powerful and sophisticated engines. Tests of NRX engines overlapped temporally with the later Kiwi and Phoebus tests. The last stage, Pewee in 1968, was an attempt to make compact engines and to test fuel compositions. Dewar (2004:174–177) also mentions an XE engine, which was tested late in the project's history. The reactor and engine tests were performed on test stands, and none ever powered a rocket.

Project Rover technologies were designed and constructed at Los Alamos and, especially in later years, at the facilities of several major contractors (Fig. 1). Investigators at Los Alamos also undertook numerous subsidiary projects and analyzed reactor components after tests. Many sites at Los Alamos have been described and assessed in compliance reports crafted by historians and architectural historians (McGehee and Garcia 1999; McGehee et al. 2004, 2009, 2010). Although apparently lacking archaeological input, the reports manage to make explicit Project Rover's footprint on the landscape because they contain maps, aerial photographs, myriad architectural drawings, historical and contemporary images of structure exteriors and sometimes interiors, and descriptions of the structures' functions and any alterations. Some structures, the reports note, had been built before Project Rover but were remodeled. Recall that Los Alamos is a vast complex, consisting of myriad Test Areas (TA) and associated structures situated on many mesa tops. Work on Project Rover was conducted mainly in buildings at TA-18 (the Pajarito site), where investigators designed the reactors in their varied iterations, carried out experiments, built many

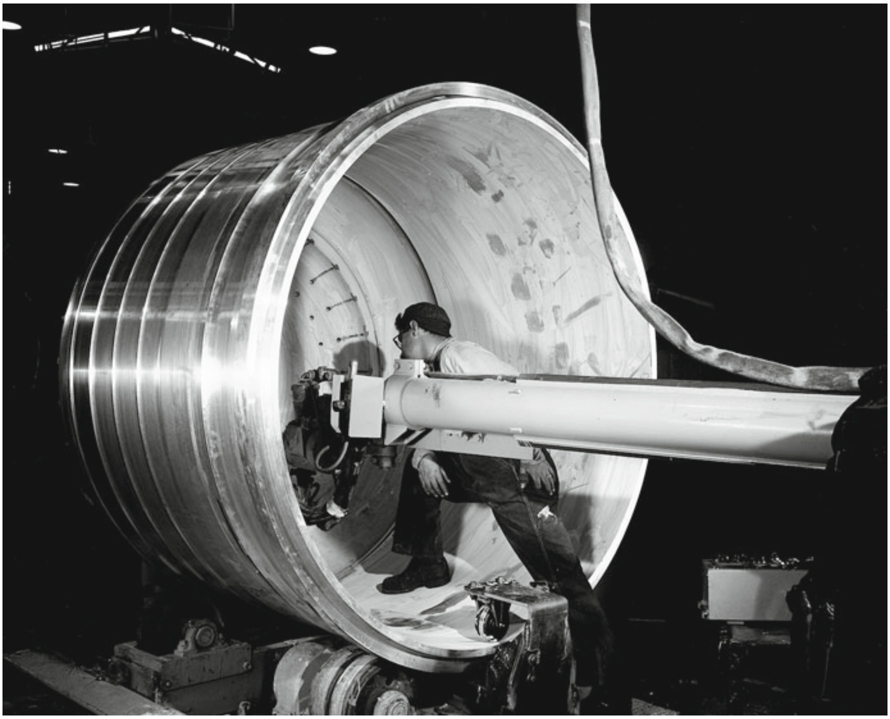

Fig. 1 Welding the body of a Kiwi A reactor in Albuquerque (courtesy of Los Alamos National Laboratory)

components, and conducted low-power tests, sometimes on mock-ups (McGehee et al. 2009). Work on fuel elements was also conducted in structures at TA-21 and TA-46 (McGehee and Garcia 1999:43; McGehee et al. 2004).

Although few pieces of equipment from Project Rover remain in Los Alamos structures, by employing use histories as well as historic photographs of interiors, it should be possible to infer activities and equipment in the spaces depicted on the architectural drawings.

Tests of Project Rover reactors and engines were conducted in Area 25 (originally Area 400), located in the southwestern portion of the Nevada National Security Site in an area called Jackass Flats. Like Los Alamos, this area is today controlled by the Department of Energy. Activities included final assembly, testing, and post-test disassembly and examination (Fig. 2); the reactor's fuel components were encased in shielding and sent to Los Alamos for detailed analysis.

Archaeologists, assisted by an architect, architectural historian, and professional photographer, described and assessed four facility complexes in Area 25, in some cases before anticipated demolition.

The earliest assembly and maintenance facility, R-MAD (Reactor Maintenance and Disassembly), was located in building 3110, constructed for that purpose in 1958 (Drollinger, Goldenberg, and Beck 2000b); associated support buildings also received some survey coverage. Encompassing some 61,290 sq. ft. on several levels,

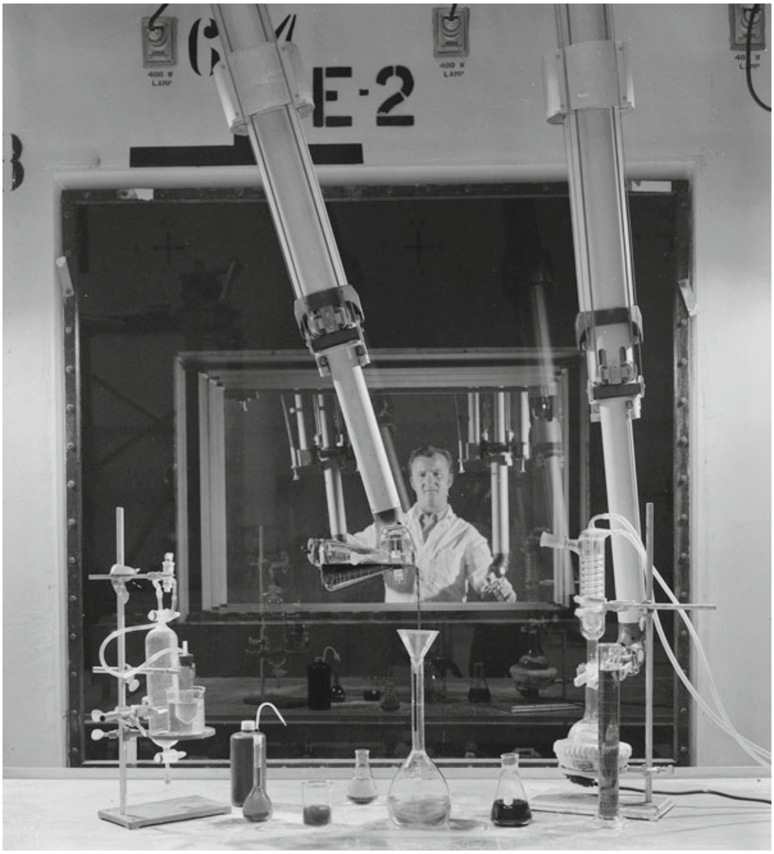

Fig. 2 Technician using manipulator arms for chemical analysis of reactor materials, ca. 1969 (courtesy of Library of Congress Prints and Photographs Division)

R-MAD was described and photographed inside and out. The report is generously illustrated, including some historic photographs and floor plans. Researchers divided R-MAD into "three functional sections: an administrative area, the assembly area, and disassembly area" (p. 7). These areas contained "offices, shops, restrooms, assembly and disassembly bays, hot cells, viewing galleries, and work stations" (p. 7). Although part of the structure was reused after Project Rover ended, the images reveal that much original equipment remained, including electronics-intensive control rooms and work stations, heating and cooling system, a cavernous hot room with equipment for disassembling the highly radioactive reactors after testing, and, throughout, many unidentified installations. Drollinger, Goldenberg, and Beck (2000b:1) recommended that R-MAD be considered eligible for nomination to the National Register. On April 8, 2010, it was demolished.[2]

[2]For a video of the demolition of R-MAD, see http://www.youtube.com/watch?v=xvC1rc3Sd4M, accessed 21 February 2012.

Although R-MAD no longer exists—except for traces visible on aerial images and as debris somewhere—the availability of historic photographs and engineering records (Drollinger, Goldenberg, and Beck 2000b furnish inventories of both), floor plans, technical reports, reports to Congress, and the compliance report's detailed architectural descriptions would enable an archaeologist to offer an apparatus-rich reconstruction of the flow of activities in the assembly and disassembly of engines that could be integrated with the research and development activities taking place at Los Alamos.

At R-MAD, the Kiwi project refined and tested reactor designs, validating that it was possible to make a nuclear-thermal reactor with sufficient thrust to propel a rocket. Phoebus project activities, in developing more powerful engines capable of longer operation, took place mainly in a new facility, E-MAD (Engine Maintenance and Disassembly). E-MAD is a windowless complex similar in functions to R-MAD but larger at 75,000 sq. ft.; it was constructed during 1962–1965 at a cost of more than $50 million. After Project Rover ended, E-MAD was reused to test concepts for handling and packaging spent fuel from commercial reactors (Beck et al. 1996:40). Planning to lease part of E-MAD for a commercial aerospace venture in the mid-1990s, the Department of Energy commissioned a historical evaluation in anticipation of decontamination activities.

The archaeological and architectural survey of E-MAD (Beck et al. 1996) and ancillary structures found that much original equipment remained, including giant manipulator arms in the disassembly area, master control center with electronics, several rooms with control panels, and machine shop with tools. Also found were boilers and electrical equipment, emergency generator, a locomotive and specially designed rail car for transporting completed engines to and from the test cell 2 miles away (Fig. 3), blast doors, and many unidentified apparatus and installations. Researchers documented the rooms with functional descriptions and 107 contemporary photographs, and recommended that the E-MAD complex "be considered potentially eligible" for nomination to the National Register (Beck et al. 1996:4). On the Internet are many aerial photographs of the complex taken at different times.

In addition to surveys of R-MAD and E-MAD, researchers were hired to describe and assess two test cells, A and C, to which the reactors and engines were transported by rail for tests and brief periods of high-power operation. Test Cell A consists of the main building (3113/3113A) of 4,390 sq. ft. and more than a dozen ancillary structures, including an enormous dewar for holding liquid hydrogen, a bunker, and a tank farm. The survey, with special attention devoted to the main building, was carried out by Beck, Drollinger, and Goldenberg (2000), who point out that this complex was "the first nuclear rocket reactor testing facility in the United States" (p. 13); the report (pp. 14–16) lists the Kiwi and NRX tests conducted there from 1959–1966 (Fig. 4). They provided a room-by-room inventory, descriptions of the architecture and remaining equipment, maps and drawings of the entire complex, index to a database of engineering drawings at the Engineering Records Library (Mercury, Nevada), and contemporary photographs showing a vast amount of equipment. In addition, there is an inventory of more than 600 historic photographs archived at the Remote Sensing Laboratory, Bechtel Nevada.

Fig. 3 Railroad moving Phoebus 2-A, April 1968 (courtesy of Los Alamos National Laboratory)

The last tests conducted at Test Cell A demonstrated that a nuclear-thermal engine could start on its own and run at full power (Fig. 5). At the time of the survey, "Test Cell A has remained untouched since its deactivation in 1966 and retains its integrity" (Beck, Drollinger, and Goldenberg 2000:18). This complex was a remarkable find, essentially a time capsule whose further study might reveal many new details about Project Rover—assuming that the Department of Energy has not destroyed it.

Test Cell C, built in 1961, had a larger main structure (10,350 sq. ft) and greater capabilities than Test Cell A; it also included ancillary structures (Drollinger, Goldenberg, and Beck 2000a). During its lifetime Test Cell C underwent many additions and modifications, in part to handle newer reactor and engine designs. Tests were carried out from 1962 to 1972, including many at full power (Drollinger, Goldenberg, and Beck 2000a:Table 1). After Project Rover's demise, Test Cell C was reused by the U.S. Geological Survey for the Yucca Mountain Project and in the 1990s by the military "to practice infiltration and urban warfare tactics" (p. 14).

Drollinger, Goldenberg, and Beck (2000a) surveyed Test Cell C, focusing on the main structure (Building 3210). The research procedures and information gathered were similar to those for Test Cell A. Photographs in the report suggest that, despite later uses, some original equipment remained, including infrastructure.

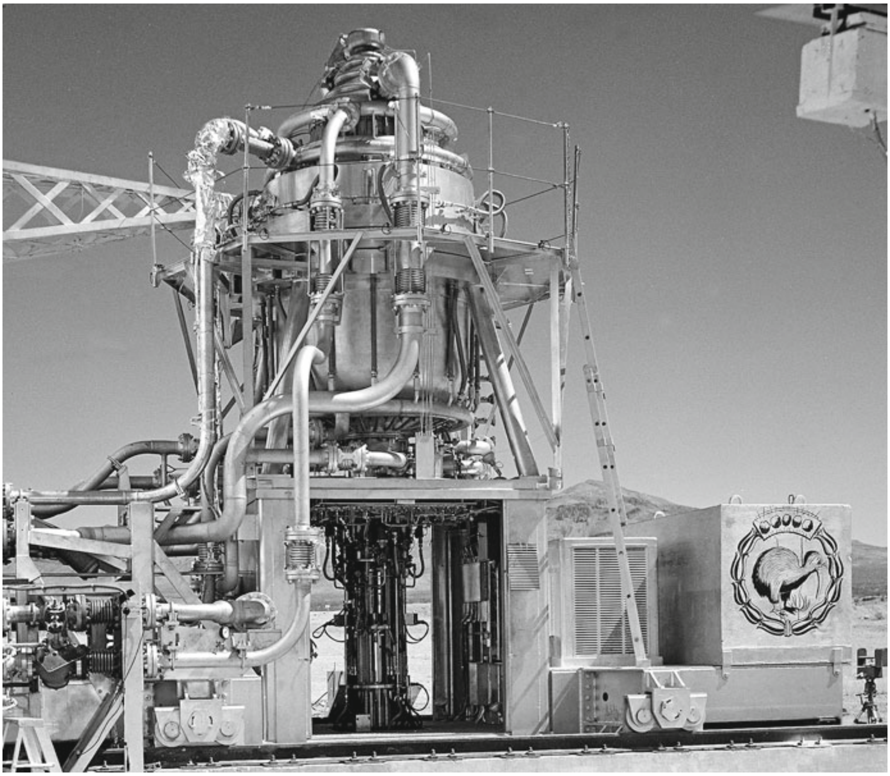

Fig. 4 Kiwi A reactor at Test Cell A, 1964 (courtesy of Los Alamos National Laboratory)

Engine tests at Test Cells A and C were conducted immediately adjacent to the main buildings (Fig. 6). Because of the dangers of radioactive exhaust and a runaway reactor, the tests were operated from the Remote Control Point, a building complex almost 2 miles away, which received data from instruments in the test cells, transmitted by cables through a tunnel. These precautions paid off: in one test, turbulence caused by the hydrogen flow ejected parts of the reactor core, spewing radioactive materials, but no one was hurt (Spence 1968). At this writing, the Remote Control Point has not been surveyed.

Some Research Questions

Regardless of which—if any—Project Rover structure complexes survive decontamination and avoid destruction, the CRM reports vividly bring to light the fascinating architectural dimension of the project and some of its unique technologies; these reports also hint at much untapped research potential that may be realized

Fig. 5 Full power test of Kiwi B at Test Cell A (courtesy of Los Alamos National Laboratory)

through study of documentary materials, drawings, and photographs in various archives. And because many Project Rover investigators are alive, oral history also offers exciting possibilities (Fishbine et al. 2011; Sandoval 1997).

One obstacle to realizing this research potential and to appreciating Project Rover as an integrated but ever-changing complex of activities is that, owing to a lack of vision by Department of Energy officials, compliance reports are prepared separately for Los Alamos and the Nevada National Security Site. Thus, any project's activities and their material expressions are handled by different research groups: in-house staff at Los Alamos and the Desert Research Institute for the Nevada National Security Site. This lack of integration suggests a need to fashion an archaeologically oriented synthesis of Project Rover (and perhaps other projects) that exploits all relevant sources of information, regardless of provenance. What would make this synthesis *archaeological* is a focus on the unfolding over time of one or more subsidiary technology and science projects—their organization, activities, architecture and places, and apparatus. Perhaps support for such research might be sought from the federal agencies that funded Project Rover (e.g., NASA) and the recent compliance reports (e.g., Department of Energy), or even major contractors such as Westinghouse, Aerojet, General Electric, and Rocketdyne that supplied some components for the engines and tests.

A technology project of considerable complexity, Project Rover generated an enormous amount of new science. Because no nuclear reactor had been used in

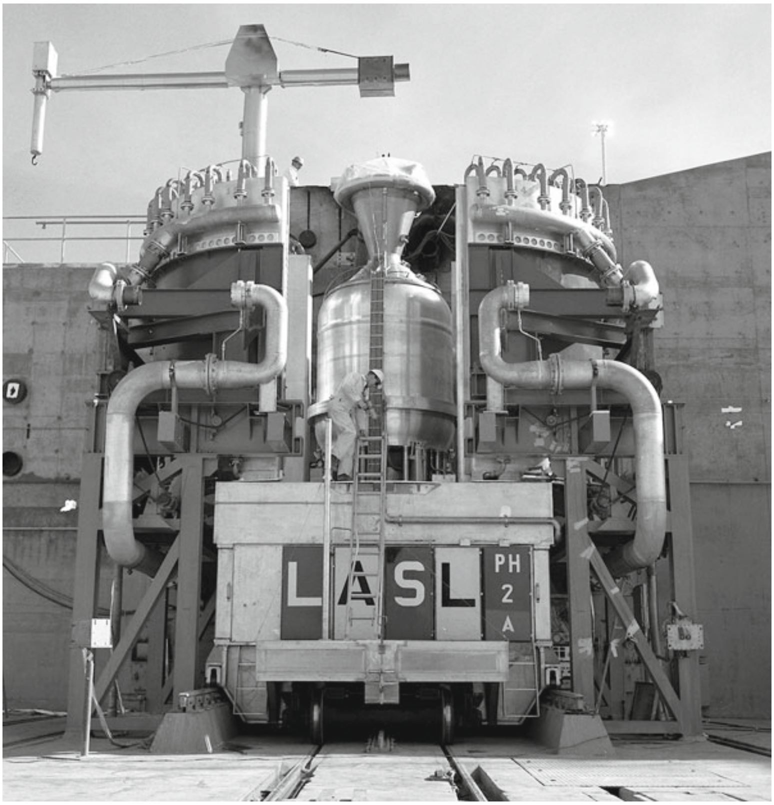

Fig. 6 Phoebus 2A and its coolant shroud at Test Cell C, 1968 (courtesy of Los Alamos National Laboratory)

this way before, many gaps in knowledge were identified early on and guided experiments. As Schreiber (1958:70) noted, "There was no backlog of basic information to serve as a foundation for specific hardware development. This lack was particularly apparent in the fields of high-temperature chemistry and physics and in the practical knowledge of the physical properties of materials at elevated temperatures." Indeed, the behavior of materials such as tungsten, molybdenum, and graphite as they approached 2,000°C had to be learned. The basic experiments were carried out at Los Alamos and required "a substantial investment in specialized equipment…[such as] resistance and induction furnaces, molding and extrusion presses, powder-metallurgy equipment, high-temperature equipment for the measurement of physical properties and specialized test equipment for subjecting reactor components to suitable conditions of temperature, pressure and gas flow" (Schreiber 1958:72). It would be of interest to track down any surviving equipment from these experiments. Even if little survived, we could draw upon archival sources, oral history, and technical reports

to construct a narrative about the ramifying series of experiments that helped to answer the basic questions.

During its lengthy life, Project Rover involved the participation of myriad organizations across the USA. Universities, government agencies, and corporations all took part in creating and testing this unique technology. Although "big science" and the "military–industrial–academic" complex have become commonplace descriptors of massive post-World War II government projects, we know precious little about the extent that such projects, and the subsidiary projects they generated, were connected materially to activities in dozens of cities and towns. I suggest examining Project Rover in this light, reconstructing in detail the behavioral chains of the materials and apparatus that entered and exited Los Alamos and the Nevada National Security Site over time.

Once Project Rover was underway, the major contractors in turn subcontracted with 30 firms in 15 states (Dewar 2004:216–217) to develop specialty materials, components, apparatus, and processes; and these—I emphasize—embodied new generalizations, especially experimental laws (e.g., about material properties) and recipes. Indeed, Project Rover created "understanding[s of] the properties of and fabrication techniques for many specialty metals…at the extremes of temperature and in a radiation environment" (Dewar 2004:217). In addition, new sensor and instrument designs yielded many technologies for measuring "temperature, pressure, radiation, and flow" (Dewar 2004:218). Any specific contribution, such as processes for producing graphite with specific properties or devices for measuring temperatures in excess of 2,000°C, could become the focus of an archaeological project.

The dispersal of subsidiary projects in many states is an important strategy sponsor agencies use to cultivate constituencies that will strongly support, in the political arena, a particular large-scale project. Project Rover's government sponsors were not the first to employ this well-known strategy, but the project might be a convenient entry point into researching the history and materiality of this strategy.

Dewar (2004:212–221) argues that many technological resources created by Project Rover transferred to space, military, and civilian projects, sometimes leading to the growth of entirely new industries such as cryogenics and robotics. The touting of so-called spinoffs of government-supported projects is sometimes little more than a public relations exercise, but Dewar makes a strong case that Project Rover did furnish resources that were incorporated into subsequent technological and scientific developments. Nonetheless, there is ample room for continued research. We could, for example, engage just one new resource and trace its post-Rover development and adoption patterns. Moreover, we could investigate whether the adopters could have acquired the new resource in other ways. In pursuing such research, we might be able to introduce nuances into facile discussions about spinoffs.

Finally, there is a hypothetical question: could a nuclear-thermal engine realistically have been used on a spacecraft? I raise this question because of my impression that the vast bulk and weight of ancillary equipment needed for engine operation—e.g., hydrogen pump, hydrogen tank, perhaps coolant shroud—might have obviated any theoretical advantages of thermal-nuclear over chemical engines. Could a second or third-stage rocket have feasibly carried the engine and its entire infrastructure?

In overcoming numerous technical problems and remedying the lack of relevant generalizations, Project Rover managed—through a host of subsidiary projects—to achieve its major Earth-bound performance goals. Writing in *Science* as the project wound down, Los Alamos investigator Roderick W. Spence (1968:953) concluded that "Thirteen years of work have produced a reliable reactor ready for development into a flyable engine." Further archaeological research may enhance understanding of how Project Rover reached its goals.

Discussion

The sprawling US nuclear establishment, which originated in the Manhattan Project of World War II, afterward continued to expand and diversify under the spur of competition with the Soviet Union during the Cold War. Many major projects with innumerable subsidiary science projects were undertaken at various government complexes to develop and sometimes to manufacture military and nonmilitary nuclear technologies. These diverse activities have left, and continue to leave, a considerable archaeological imprint at the Nevada National Security Site and at laboratories around the country. The limited amount of archaeological and historical research conducted thus far has helped to "ground," for example, the activities of the Manhattan Project and Project Rover in actual material remains. Moreover, these compliance studies have created or found many sources of evidence, such as architectural drawings and contemporary photographs, that can be mined for further insights. In view of growing archaeological interest in World War II and the Cold War (e.g., Schofield and Cocroft 2007; Schofield, Johnson and Beck 2002), continued study of the unique archaeological and historical materials relating to Los Alamos and the Nevada National Security Site provides opportunities to contribute to the archaeology of science and also to the archaeology of "the contemporary past" (Buchli and Lucas 2001; Harrison and Schofield 2010).

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Archaeology of the Space Age

As a youngster during the 1950s I devoured science fiction, especially the works of Robert Heinlein, and watched *Rocky Jones*, *Space Ranger* on our black and white TV. Rocky's spaceship resembled the V-2 rocket, the kind that Germany fired at England and other countries during World War II. Some people place the V-2 at the beginning of the Space Age because its designers, Wernher von Braun and other Nazi rocketeers, willingly surrendered to the USA and began developing the mighty rockets that contributed to American weaponry and the space program, and ultimately built the 364-foot-tall Saturn V that lofted men to the Moon. On July 20, 1969, in two adjoining rooms of a shabby motel in Showlow, Arizona, I and the entire field camp of Paul S. Martin's Southwestern Archaeological Expedition watched intently as Neil Armstrong created at Tranquility Base the first traces on the Moon of human presence. Little did I suspect then that artifacts, facilities, and sites of the Space Age would one day be subjects of archaeological research.

Since the beginning of the new millennium, archaeologists in several countries have begun to explore this research domain, which is known as exo-archaeology (Campbell 2004; Rathje 1999), aerospace archaeology (Capelotti 2004), space archaeology (Darrin and O'Leary 2009a), and archaeology of the Space Age (Haden-Guest 1974). There is a rapidly growing literature that includes a handbook (Darrin and O'Leary 2009b), monograph (Capelotti 2010), journal articles (e.g., Capelotti 2004; Gorman 2005a, 2009a, 2009b, 2009c, 2011; O'Leary 2006), chapters in edited volumes, and websites and blogs. Symposia at national and international conferences have also included papers and posters on aerospace archaeology (O'Leary 2009). As a sign of this nascent field's growing recognition, the World Archaeological Congress formed the Space Heritage Archaeology Task Force to craft guidelines on the management of space-related cultural resources.[1] This chapter engages issues of space-heritage preservation, but its major focus is archaeological studies of early rocketry and space-related activities of the United States.

[1] http://www.worldarchaeologicalcongress.org/activities/taskforces/261-space-heritage, accessed 28 December 2012.

M.B. Schiffer, *The Archaeology of Science*, Manuals in Archaeological Method, Theory and Technique 9, DOI 10.1007/978-3-319-00077-0_12,

Motivations for Space Exploration

For many citizens of the West, the motivation for space exploration seems obvious: the human urge to venture into and explore new territories. With terrestrial frontiers on Earth apparently shrinking, the next "logical" move is to reach beyond Earth. In concert with other authors, Gorman (2005a:99) remarked that presenting a space program "as a natural inclination … concealed other motives for space exploration, such as military advantage, national prestige … and access to resources and colonization."

Many factors beyond a supposed exploratory drive impelled countries to undertake resource-intensive projects of space exploration. The two major players, the USA and the former Soviet Union, were locked in a peer competition during the Cold War (Schiffer 2011:43–44), manifest in the "space race" and the "missile race." Each country strove to take the next technological step, to leapfrog its competitor (on Cold War archaeology, see Schofield and Cocroft 2007). For every landmark achievement—e.g., first satellite, first man in orbit, first spacewalk, first lunar probe, first manned Moon landing—the triumphant country touted the superiority of its technology, society, and ideology to its adversary, citizens at home, and to people in "developing" countries. In addition, the military applications of space technologies did not go unnoticed, for decades of science fiction had immersed readers in interstellar wars. And military and space-exploration activities were interdependent because technologies developed for one domain were often adapted for the other, and many missions had both scientific and military components. This peer competition pushed technological development across a broad front and resulted in many recipes for new aerospace materials, components and products, from ceramic insulation to rocket engines to space suits. Predictably, numerous generalizations arose in subsidiary science projects.

Although macro-factors such as peer competitions help us to frame some space-exploration projects, a significant cultural factor is missing. Let us turn to the cultural imperative model of invention: "*A cultural imperative is an imagined technology believed by a group—its constituency—to be desirable and inevitable, its realization merely awaiting appropriate technological resources*" (Schiffer 2011:65, emphasis in original; see also Schiffer 1993). When members of that constituency occupy powerful positions in companies and governments, they may channel resources into developing their coveted technology. Science fiction, as materialized in books, short stories, magazines, toys, and films, helped to nurture and perpetuate a constituency of enthusiasts who dreamed of building spaceships.

Many regard Robert H. Goddard as the father of American rocketry (Lehman 1963; Winders 1963). As a teenager, he was smitten by the idea of space travel after reading H.G. Wells' *War of the Worlds*, and spent much time pondering the technological challenges. As a professor he was able to conduct rocket research, obtaining support from several universities; he also landed a grant from the Smithsonian Institution and later received funds from the US government. Goddard's experiments enjoyed successes such as the use of liquid fuels and flight controls, which were incorporated into later rocket designs.

Fig. 1 Wernher von Braun in his office, September 1, 1960 (NASA/courtesy of nasaimages.org)

Wernher von Braun, the major figure in German rocketry (Neufeld 2007; Ward 2005), was also captivated by ideas of space flight, and as a young man dreamed of voyaging to the Moon. Drawing on the technology and science of rocketry established by Goddard and others, von Braun and his team, employing enslaved labor, developed for Nazi Germany the V-2 rocket. In postwar years the US government supported his experiments, which culminated in Saturn V. Predictably, von Braun was familiar with science fiction. Not only did he decorate his office with a picture suggestive of sci-fi magazine covers (Fig. 1), but he also wrote his own novel, *First Men to the Moon* (von Braun 1960). A member of the Nazi Party, von Braun was amoral, if not immoral; he cared not who funded his experiments, nor about their heavy toll on human lives. He just wanted to build more powerful rockets and send men to the Moon.

Beyond Goddard and von Braun, many ordinary people read science fiction, watched science fiction movies and TV programs, and had visions of setting foot on the Moon or Mars. Moreover, some members of the constituency were, like Goddard and von Braun, well-educated and articulate experimenters who could acquire support for their work by inventing rationales to convince others of its importance. And no doubt the constituency also had members in positions of authority in many countries. The Cold War and its technological competitions gave these enthusiasts ideological mandates and furnished the resources to develop the rockets and support technologies that could help materialize their visions. Clearly, we can enhance our understanding of the space and missile races by recognizing the crucial roles—scientific and political—played by people who fervently believed that technologies for space travel were inevitable.

Subject Matter

What kinds of artifacts and sites do archaeologists of the Space-Age study? Darrin and O'Leary (2009a:5) offer a general definition of the subject matter: "material culture relevant to space exploration that is found on earth and in outer space… [including] all material culture in the aerospace and aeronautical realms that relate[s] to the development and support of exoatmospheric activities." Gorman (2005a:86) adds some specifics: "functioning and nonfunctioning satellites, upper rocket stages, probes, landers, modules, organic human remains, orbital debris and 'space junk' … launch facilities, tracking stations, research centers and domestic satellite dishes." Also included is everything that orbits Earth and other celestial bodies, launch and crash debris on Earth and crash debris on other bodies, artifacts in museums and in private collections (on the latter, cf. Gorman 2011; Haden-Guest 1974; O'Leary 2009), facilities and equipment for manufacturing and testing rockets and other aerospace equipment, and places for training astronauts and simulating missions. Some researchers would even include missiles, missile silos, spy satellites, and related military technologies. Spennemann (2004) provides an eye-opening list of the multitude of facilities, places, and artifacts associated with just the Apollo missions, which spanned several countries, more than a dozen states, and the Moon. Evidently, a vast amount of space-related materials resides in both systemic and archaeological contexts; and distinct landscapes holding traces of human activities are present on Earth, the Moon, other planets, and even asteroids.

Gorman (2009b:346) observes that orbiting materials are unusual from an archaeological standpoint because they "are in perpetual motion relative to our vantage point, and relative to one another." Adding to the complexity, these artifacts travel at different speeds, in different directions, and at different altitudes. And they are abundant: in Earth-orbit alone there are 15,000-plus objects greater than 10 cm, which are tracked by the U.S. Strategic Command (Clemens 2009). Orbits may be computed for any object, but they can be altered by "impacts from meteorites, high-energy particles and other space junk" (Gorman 2009b:346). Given the constant flux of orbiting materials, Gorman wonders how the spatial and temporal locations of specific artifacts can be described since traditional archaeological conventions are useless. Nonetheless, many research questions would require only the knowledge that a particular artifact resides in an earth orbit or once did.

The issue of subject matter reaches beyond artifacts and sites, for there is the problem of framing the research. The traditional Cold-War framing is useful for some studies (Gorman and O'Leary 2007), but suffers from a general problem: "the interests of largely white male American astronauts, space administrators, scientists and politicians are presented as universal human values" (Gorman 2005a:86). Drawing on three case studies—Peenemünde, the first location where V-2 rockets were developed, manufactured, and tested; the Woomera Rocket Range in south-central Australia, which served several countries; and Tranquility Base, where Apollo 11 landed—Gorman demonstrates how space exploration also impinges on the values and lives of other peoples. Archaeologists, she insists, must consider what space means to Earthlings other than the elites of space-exploration communities.

The Woomera case study is especially compelling (Gorman 2005a, 2009c). Occupying an enormous tract in south-central Australia, the Woomera Rocket Range was established in 1947 at the behest of Britain. In the range's heyday, the UK and USA used nine launch areas for military and space-exploration activities. Britons and white Australians judged Woomera an ideal location because they perceived it to be an uninhabited wasteland. As Gorman points out, however, Woomera, named for the Aboriginal spear-thrower, encompassed part of the Central Aborigines Reserve and encroached on the traditional territories of the Kokatha and Pitjantjatjara peoples. Many groups of sympathetic outsiders, including religious figures, organized protests early on, objecting to the way Australia so easily trampled the rights of its original inhabitants (Gorman 2005a:95–96). Several factors, including the ever-present dangers of living on an active missile range, led the Kokatha to abandon their country. Residing in the Central Aborigines Reserve, the Pitjantjatjara people were forced into close contact with whites, and as a result were also adversely affected.

The Kokatha and Pitjantjatjara territories were hardly desolate, for they contained flora, fauna, and other resources that had long been exploited with traditional skills and technologies. These peoples also wove the natural products and topographic features into stories that were central to their societies. Testifying to these activities, "The supposedly barren desert is scattered with the stone tools, ceremonial sites and rock art of Kokatha and Pitjantjatjara people and ceremonies related to Dreaming stories continue to be performed today" (Gorman 2005a:98).

Although the Woomera region retains significance as a traditional territory, it has accrued new meanings. Missile testing and rocket launches largely ceased in the mid-1970s, but Australia sited in Woomera other installations such as a US surveillance base. These unpopular facilities have given rise to "a landscape of continuing protest…an intangible heritage of high significance" (Gorman 2005a:98). Nontraditional uses of this landscape have entered the Aboriginal perspective as "one strand of a colonial process that led to alienation from their country and deprivation of their human rights" (Gorman 2005a:98). But, as Gorman also observes, confrontations over Woomera nurtured the growth of Aboriginal political activism and led to vigorous debates about the place of these peoples in Australian society.

Gorman demonstrates that archaeologists can help tell the story of how space-related activities affected other peoples. Indeed, the social and cultural impacts—positive and negative—of large-scale science and technology projects on *all* affected groups are little studied. Balanced research taking into account diverse perspectives through oral history, the documentary record, ethnoarchaeology, and archaeological remains would be essential for "managing cultural heritage" (Gorman 2005a:98). In this way, archaeologists could push "beyond the 'Space Race' model, which assumes a global and uniform significance for places associated with the development of space exploration" (Gorman 2005a:103).

A landscape framework appears applicable to this enterprise (Gorman 2005a); after all, landscapes are at the intersection of, for example, colonial and native groups (on landscape studies, see David and Thomas 2008; Hollenback 2010). Easily handled in this framework, the rocketry facilities in Woomera are well represented in the archaeological record. The diverse remains "include infrastructure at the nine launch

areas, of which the most impressive are the ELDO launch pads on the edge of a vast salt lake, roads, instrumentation buildings, workshops, blockhouses, security checkpoints, tracking stations (Island Lagoon, Red Lake, Mirikata) and the Woomera township which still supports a population of around 200 people" (Gorman 2005a:94). Research on the archaeological record of Woomera has already begun (Alice Gorman, personal communication, May 2012).

Research Resources

Regardless of how their research projects are framed, aerospace archaeologists have access to a dizzying array of resources, including websites of the National Aeronautics and Space Administration (NASA), space enthusiast websites and blogs, national archives, oral history, objects and documents in museums, museum curators with special expertise in aerospace objects, and thousands of books and articles in popular and technical journals. Obviously the U.S. National Archives and the Smithsonian's National Air and Space Museum possess unparalleled resources as do other museums around the country, especially the U.S. Space & Rocket Center in Huntsville, Alabama, the Kennedy Space Center Visitor Complex on Merritt Island, Florida, and the White Sands Missile Range Museum in New Mexico (on the latter, see below). The Russian space program and that of the former Soviet Union are documented in stunning exhibits and informative websites.[2] Other countries, including some that never sent a payload into space, also have national and local museums with noteworthy holdings.[3]

In the 1980s, the U.S. National Park Service undertook a two-phase project that identified space-related sites on Earth that should be designated as National Landmarks or nominated to the National Register of Historic Places. This list includes nationally significant research, development, and support facilities across the country as well as museum objects such as used space capsules (Butowsky 1984, 1986).

The major US military bases that hosted missile and rocketry activities have on-site museums and archives as well as environmental divisions that attend to cultural resources; and several have staff archaeologists. Three bases have seen the majority of state-side development and launch activities: White Sands Missile Range (formerly White Sands Proving Ground) in New Mexico, Cape Canaveral (under the jurisdiction of Patrick Air Force Base) in Florida, and Vandenberg Air Force Base in California. Some archaeological fieldwork has been conducted on all three bases, but the focus here is on White Sands, "the scene of pioneering efforts in missile-systems testing, space biology, guidance, telemetry, meteorology, and atmospheric science" (Eidenbach et al. 1996:2).

[2] For example, http://englishrussia.com/2009/06/10/the-russian-space-museum/, accessed 30 May 2012; http://www.russianspaceweb.com/site_map.html, accessed 30 May 2012.

[3] This site is a "guide to great space exhibits and museums": http://www.museumofspacetravel.com, accessed 30 May 2012.

White Sands Missile Range

Human Systems Research, Inc. has done the bulk of archaeological studies on the White Sands Missile Range, including an extensive overview and field inspection of sites (Eidenbach et al. 1996) and excavation of a domestic dump used during 1945–1947 (Duran et al. 1997). Also of interest is the White Sands Missile Range Museum, operated by the White Sands Missile Range Historical Foundation and chartered to preserve and present White Sands history to the public. And that history is central to understanding the development of US missiles and related technologies because of the extensive tests carried out there, which included the firing of V-2 rockets, many with scientific instruments aboard. The general outlines of V-2 work are well known from documentary sources, museum specimens, and books, but there is ample potential for archaeological research.

Immediately after the war, von Braun and a group of 118 German scientists and engineers as well as parts for about 100 V-2 rockets, other equipment, and 12 tons of documents were carried across the sea in 15 Liberty ships and over land in 300 rail cars to a sparsely inhabited region of south-central New Mexico that had already been used for military activities (Eidenbach et al. 1996:5; Enscore 1998). Part of "Operation Paperclip," most of the Germans lived on the Army's Fort Bliss, nearby in El Paso, Texas, where they practiced English, conducted research, and continued V-2 development. Although some documentary materials along with von Braun were relocated in 1950 to the Army's Redstone Arsenal in Alabama (part of which in 1960 became the Marshall Space Flight Center under NASA), the White Sands museum contains a plethora of declassified documents and artifacts, including a refurbished V-2 rocket motor, V-2 hydrogen peroxide tank, V-2 gyroscope, German circular slide rule for calculating rocket trajectories, and German mechanical calculator for reducing data from test firings. There is also an online archive of documents and photographs that includes materials related to von Braun's tenure at White Sands.[4]

The Museum Website has a searchable database of missiles tested there, which provides the first firing date and basic technical information. One example is the Aerobee series, prototypes of which were first fired in 1947. James Van Allen, who is credited with discovering the radiation belt surrounding the earth that carries his name, had a hand in designing this rocket; one model was 31 ft long and could reach a height of 165 miles. With its instrument package it probed the characteristics of outer space, furnishing information for military and space-exploration applications.[5] Outside the museum structure itself is the White Sands Missile Range Missile Park, which exhibits a phalanx of dozens of missiles, including a refurbished V-2 as well as many intriguing one-off artifacts.

To comply with the Legacy Program and other federal legislation, the Department of Defense undertook a demonstration project to identify National Register-eligible

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[4] http://www.wsmr-history.org/Archives.asp, accessed 30 May 2012.

[5] http://www.wsmr-history.org/Aerobee170.htm, accessed 30 May 2012.

properties at White Sands related to the historical theme of the Cold War. Mainly covering the period 1942–1964, the researchers made extensive use of unpublished histories, oral history, and documents to fashion a detailed and well-illustrated history (Eidenbach et al. 1996). Each property—e.g., "isolated launch pads, blockhouses, test sites, and instrumentation stations" (p. 1)—was recorded in the field and the structural remains compared to engineering drawings.

The report begins with a detailed year-by-year account of activities, which describes the expansion and consolidation of the base and its use for Army, Navy, and Air Force missile programs. The next section discusses each missile program, giving relevant dates, achievements, and mentions the companies involved in missile development and manufacture along with the universities participating in the research component of many projects. Then follows a section that describes each surviving property under the following "thematic groups": Army Infrastructure, Lab/Assembly, Navy Infrastructure, Launch Complexes, Static Test Stands, Instrumentation, and Range Camps (p. 112).

Of the more than four dozen Cold War-related sites at White Sands, I mention just two: Launch Complex (LC) 33 and the 100-K Static Test Facility. Construction of LC-33 (Fig. 2) began in mid-1945, making it the oldest major US launch complex (pp. 137–144). From LC-33 were sent aloft 67 V-2 rockets (Enscore 1998:25) and, after some modifications, several US-built missiles. This complex consists of many structures, including two blockhouses, gantries, missile storage facility, explosive-storage bunkers, and concrete pads; and many—especially the gantries—retain their integrity. LC-33 is now on the National Register of Historic Places.

Before launch, a rocket's motor was fired in a static test stand, which held the rocket in a stationary position at full thrust. In this mode, the motor and the rocket's mechanical stability could be monitored. The 100-K facility, consisting of the stand itself and a control building, was built in 1946 for the V-2 program; it could restrain a rocket generating 100,000 lb of thrust. Both the stand and control building "remain in excellent condition" (p. 155).

This report also contains a discussion of oral history and an appendix, organized by missile name, that lists test firings from 1945 to 1964 (p. 225). The different missiles and varieties are named along with the numbers fired; in total there were many thousands.

The White Sands Missile Range is no longer active, but for archaeologists interested in early US missile and rocket development, it holds much relevant evidence, especially in the museum's large document collection and surviving structures and artifacts.

Discussion

The increasing availability of information about, and sometimes access to, early missile- and rocket-related sites on military bases gives us the opportunity to create archaeological syntheses of many previously secret technologies and their

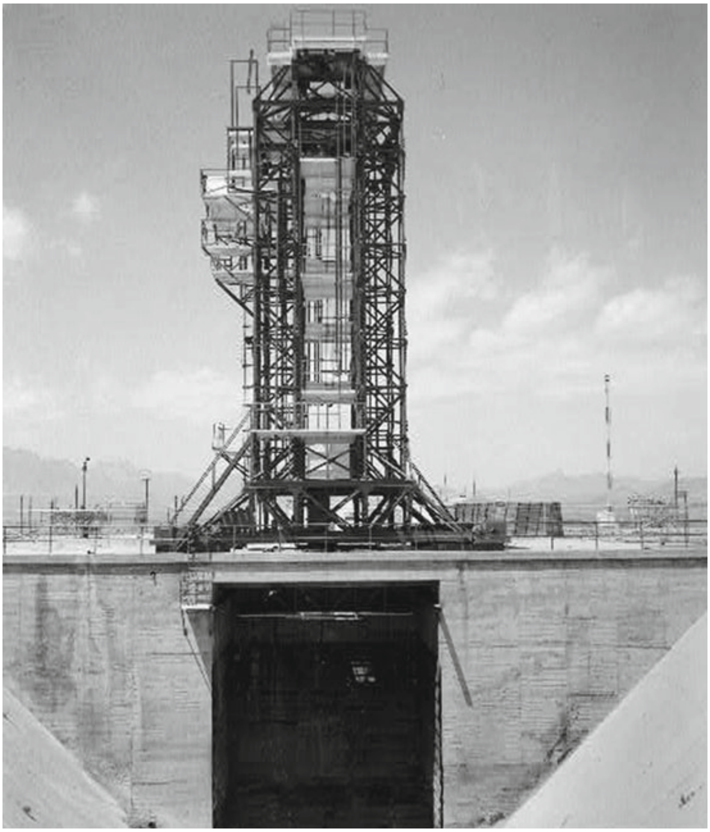

Fig. 2 Launch Complex 33, White Sands Missile Range, New Mexico (courtesy of Library of Congress, Prints and Photographs Division, Washington, DC)

subsidiary projects. A study of the V-2 program's archaeological remains in the USA is now eminently feasible, and would at the very least include the White Sands Missile Range, the Cape Canaveral complex, and Fort Bliss, supplemented by copious documents, museum artifacts, and technical reports.

Four major impressions remain from my superficial inquiry into the US government's early involvement in missiles and rocketry. (1) To a large extent the military projects incorporated terrestrial, atmospheric, and biological science experiments whose findings contributed to the space program. (2) In development and testing activities, the Air Force, Navy, and Army competed vigorously, which led to partially redundant projects. (3) Testing activities took place on military bases in many states; manufacturing activities by aerospace corporations were likewise dispersed. (4) Although there has been an enormous amount of archaeological and historical compliance research on military bases, the reports reside almost exclusively in the gray literature and are sometimes difficult to obtain.

Preservation of Sites and Artifacts in Space

Because a site's research potential depends in part on its degree of integrity, archaeologists have made the case for preserving the remains of space exploration. Another motivation for preservation is that of heritage values, for these sites and technologies are significant to various groups, especially the aerospace community, and so may merit protection (Barclay and Brooks 2002; London 1993; Spennemann 2004). Most Americans, for example, would be appalled if space tourists someday collected artifacts from Tranquility Base and trampled the Apollo astronauts' footprints. Archaeologists and other groups have galvanized interest in identifying legal avenues for preservation (e.g., Fewer 2002; Gibson 1999; O'Leary 2006; Walsh 2012). The Lunar Legacy Project at New Mexico State University, under the leadership of Beth O'Leary, has been especially effective at publicizing this issue.[6] With space tourism looming in the years ahead, the preservation issue has become pressing.

Unfortunately, lunar sites lack legal protection (Fewer 2002). Although the US government retains ownership of the artifacts that its missions deposited on the Moon, and so in principle could prosecute souvenir collectors, the surface on which they rest—including footprints and vehicle tracks—cannot, by provisions of the Outer Space Treaty, be owned by any nation (Fewer 2002; Spennemann 2004). The long-term solution would be an international agreement barring disturbance of sites such as the treaty that governs historic sites in Antarctica (Walsh 2012).

NASA has devised a stopgap measure. After protracted deliberations in response to pressure by archaeologists and others, NASA in May of 2012 issued a set of guidelines for organizations planning Moon exploration. This move's ostensible purpose is to give guidance to the 26 teams from more than a dozen countries vying for the Google Lunar X-Prize, which will be awarded to the first team that lands a privately funded rover on the Moon.[7] The guidelines may discourage X-Prize competitors from artifact collecting and site disturbance, but others might not be deterred.

Gorman (2005b) offers an intriguing argument for leaving some objects in Earth orbit, thus preserving them in a "spacescape." She acknowledges that abandoned satellites, rocket parts, and so forth are collision hazards, but also notes that a small number of large satellites present a trifling risk compared with the many thousands of medium-size objects that are hence more likely to collide with functioning spacecraft (Gorman 2009d). Several strategies for dealing with orbital debris, beyond minimizing it initially through spacecraft design and enlightened operation, have been discussed: (1) targeting small debris with lasers from Earth or space, (2) employing spacecraft to pluck old satellites from orbit, perhaps returning them to Earth or sending them into a decaying orbit where they will be incinerated, (3) commercial salvage, and (4) moving debris to very high ("graveyard") orbits (Gorman 2005b, 2009d; O'Leary 2009; Osiander and Ostdiek 2009).

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[6] http://spacegrant.nmsu.edu/lunarlegacies/index.html, accessed 30 May 2012.

[7] Guidelines: http://go.nasa.gov/JDYo9v, accessed 30 May 2012; the list of teams: http://www.googlelunarxprize.org/teams, accessed 30 May 2012.

Before any of these strategies is implemented on a large scale, Gorman insists that the heritage values associated with specific objects be assessed. She suggests that dead satellites in particular "may have social, historical, aesthetic, and scientific significance for nations, communities, groups, and individuals who will have an interest in decisions made about their long-term survival" (Gorman 2009d:382). One example is Syncom 3, a US satellite launched in 1963. In geostationary orbit, Syncom 3 and its sister satellites inaugurated the technologies that sent telephone messages and live television around the globe. Communication and reconnaissance satellites are now an enormous commercial enterprise with some ancestral objects still in orbit. Another example is Vanguard I, a US satellite with a scientific mission launched in 1958. Because it is the oldest satellite still in orbit, Gorman maintains that it has a significance there that it would lack on Earth. Syncom 3, Vanguard I, and other early satellites (see Gorman 2009d: 390–391 for additional examples) are clearly significant artifacts, but Gorman argues that they should stay in orbit because of the special heritage values that they retain in space.

Yet, Gorman does acknowledge that objects retrieved from space, such as Mercury and Gemini capsules, draw huge crowds at the National Air and Space Museum, and so are meaningful to many people from many countries. If Syncom 3 and Vanguard 1 were returned to earth and put on exhibit, they too would likely elicit much interest. Left in space, however, they would be meaningful only to a small group of cognoscenti immersed in aerospace history. Although Gorman's argument for preserving parts of the spacescape has many merits, this strategy denies ordinary people the opportunity to learn *something* about early satellites by direct observation. And, if brought back to earth, these artifacts would be available for researching topics such as deterioration processes as well as the nature and provenance of materials and parts used in early satellite manufacture. Owing to its great expense, however, the retrieval option is likely to remain moot for a very long time.

Lunar and Planetary Archaeology

The First Recovery of Lunar Artifacts

According to one estimate, there are more than 80 sites on the Moon (O'Leary 2009). Although conducting actual fieldwork on the Moon is apparently out of the question now and for the foreseeable future, the Apollo 12 astronauts carried out the first archaeological recovery of lunar artifacts (Capelotti 2004, 2010:19–20, 34; O'Leary 2009). Their quarry was Surveyor 3, an unmanned probe that reached the Moon on April 20, 1967. In its short uselife—it died during the first lunar night—the probe transmitted thousands of video images and, with a scoop on an extendable arm, tested properties of the lunar surface.

On November 19, 1969, the Apollo 12 Lunar Module landed about 155 m from Surveyor 3. This close encounter was no accident, for NASA wanted to show that a

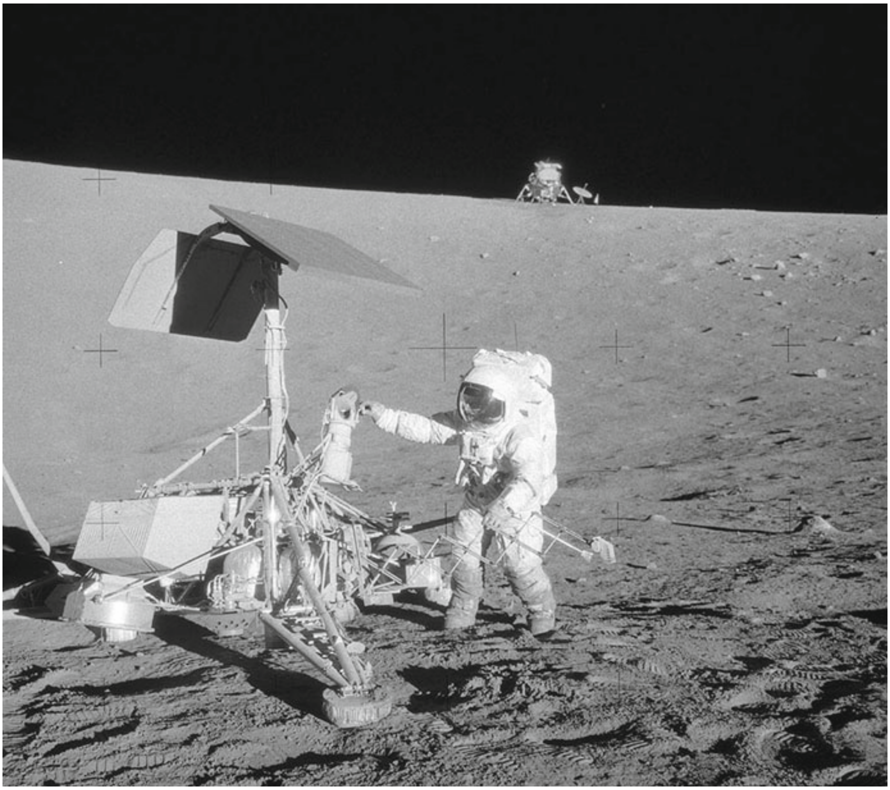

Fig. 3 Astronaut Alan L. Bean inspects Surveyor 3 on the Moon. His right hand is on the video camera; his left raises the scoop's arm (NASA/Courtesy of nasaimages.org)

specific landing site could be precisely targeted; there was also keen interest in obtaining information from photographs of the landing site and retrieving several spacecraft parts. Indeed, this was a study of how environmental processes affected Surveyor 3's materials and components.

After taking many photographs and observing the condition of Surveyor 3, Alan L. Bean and Charles Conrad, Jr. used cutting shears to remove representative materials (Fig. 3): (1) the entire video camera including optics, electronics, and mechanical components, (2) the soil scoop and its contents, (3) a 20 cm piece of aluminum strut, (4) a 10 cm piece of aluminum tube which had been painted white, and (5) 13 cm of video cable (Carroll et al. 1972:3–5). The artifacts were carefully bagged to avoid damage and contamination, but some bag wear did occur (p. 5). As in any modern archaeological project, the Surveyor 3 specimens were studied by teams of specialists from many disciplines employing physical and chemical characterization techniques, and their findings were reported in a lengthy monograph (NASA 1972).

The most general finding was that "no failures or serious adverse environmental effects on the hardware were uncovered that, to some degree, had not been

anticipated" (Nickel and Carroll 1972:9). Laboratory simulations, for example, had indicated that solar radiation would cause a darkening of exposed components, and this was observed. Deposition of radionuclides on specimen surfaces was measured and found to be in agreement with previously estimated values. Bombardment by particles emanating from cosmic radiation, solar flares, and the solar wind had no effects on the spacecraft's microstructure. Somewhat unexpected, however, was the coating of tiny dust particles adhering to various surfaces. The dusting, it was inferred, occurred mainly during Surveyor 3's awkward landing and the Lunar Module's descent. The investigators also sought traces of micrometeorite impacts but found no definitive pits; and photographs of the lunar surface taken near Surveyor 3 revealed no impact craters greater than 1.5 mm in diameter (Nickel and Carroll 1972:11). Curiously, on the camera's interior was found a viable bacterium, *Streptococcus mitis*, no doubt the result of pre-launch contamination.

The NASA monograph also makes recommendations for future research. Although acknowledging that Surveyor 3 was not designed to provide scientific samples for a later expedition, Nickel and Carroll (1972:13) lament "the lack of suitable controls, standards, or documentation of initial conditions," and advise greater attention to controls in future projects. Toward that end the authors suggest that spacecraft include "a set of coupons consisting of different types of material of interest" (p. 13), which would be inexpensive and could be deployed remotely (a coupon is a sample of material with specified properties that can be deployed in a test environment and compared with identical controls).

Remote Sensing of the Lunar and Martian Surfaces

Despite the expectation of future projects implicit in NASA's report on the Apollo 12's archaeology mission, no additional human visits to lunar sites have taken place. A more feasible and nondestructive option for documenting the traces of Moon exploration is available. On June 18, 2009, NASA launched the Lunar Reconnaissance Orbiter (LRO). The LRO's mission, in preparing for new visits by US crews, first to the Moon and then to Mars, is to "find safe landing sites, locate potential resources, characterize the radiation environment and test new technology."[8] In addition to LRO's stated goals, there is, perhaps, an unstated one: to undermine the conspiracy theorists who claim that NASA faked the Moon landings (cf. Capelotti 2010:1).

A remote-sensing platform containing seven instruments, the LRO orbits about 50 km above the lunar surface, but the orbit can be lowered. Among the wealth of data returned to Earth are stunning images of the Moon's surface, especially the Apollo landing sites. These sites consist of abandoned and discarded objects, including lunar rovers from the last three missions, the descent stage of the Lunar Module, and instrument packages.

[8] http://www.nasa.gov/mission_pages/LRO/overview/index.html, accessed 5 June 2012.

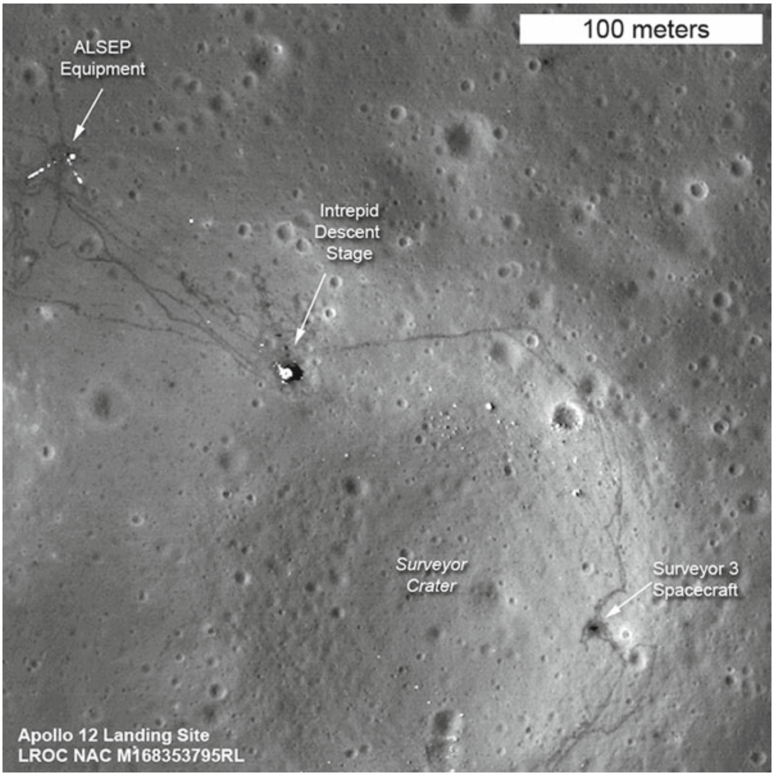

Fig. 4 The Apollo 12 landing site on the Moon as seen by the Lunar Reconnaissance Rover (NASA/courtesy of nasaimages.org)

Figure 4 is an image of the Apollo 12 landing site taken from a close LRO flyby. On this image are labeled: (1) ALSEP, the instrument package that Apollo 12 left behind, (2) Surveyor 3, and (3) the Lunar Module's descent stage. Also visible are the astronauts' footpaths, which appear as thin, dark meanders. Clearly, the LRO has furnished, and continues to furnish, the kinds of images that document the macro-archaeological record of the Apollo program.

After inspecting LRO images of the Apollo landing sites, Capelotti (2010:3, 5) offered an explanation for the sinuous footpaths: "Given a flat, largely unobstructed surface, the paths are relatively straight. When confronted by a landscape of mounds and craters, the humans go around these obstacles rather than across or over them." Capelotti suggests that earthly behavior patterns were thus replicated on the lunar surface. Another interpretation is that the astronauts in their cumbersome space suits were very cautious and avoided climbing or descending inclines.

In 1996, the USA launched the Mars Global Surveyor, a craft that operated for a decade, returning data and images from the red planet and photographing its surface

in search of future landing sites. Cameras also captured the landing sites of the American rovers, Opportunity and Spirit, and the stationary probe, Viking I.[9]

The LRO, Mars Global Surveyor, and other satellites are furnishing usable images of landing and crash sites on the Moon and Mars. Indeed, lunar crash sites seem to have a distinctive visual signature, a patch darker than its surroundings, caused by the formation of an impact crater that redeposits darker subsurface sediment on the surface (Stooke 2009). Given the relative ease of remote imaging, archaeologists might recommend to NASA which lunar and Martian sites to photograph at very high resolution.

The Extraterrestrial Archaeological Record

Although unable to visit sites beyond earth, archaeologists enjoy some advantages in studying space exploration because of the extraordinary amount of documentation that has been compiled for every mission (Capelotti 2010:5). Drawing on these sources, Capelotti (p. 11) has assembled data on the artifacts that humans have "placed, lost, used and/or abandoned on celestial bodies…other than Earth." His monograph has three parts: Lunar Archaeology, Planetary Archaeology, and Interstellar Archaeology. For each mission, success or failure, Capelotti used NASA sources to provide a thumbnail history, technical details, and general statements about the kinds of artifacts deposited. His description of the Apollo 11 archaeological record, for example, includes the following specifics: "the descent stage/launch pad, video and still cameras, scientific sampling tools, discarded life support systems, an American flag, and several remotely operated scientific instruments, including a laser beam reflector, seismic detector, and a gnomon, a device to verify colors of the objects photographed" (p. 34).

Capelotti also lists archaeological remains on Venus, Mars, and Saturn's moon Titan, which include probes; on Mars there are also robotic rovers (see Gold 2009 for mention of artifacts on other bodies). Also discussed are dozens of spacecraft orbiting the sun (pp. 165–166), a few headed to outer planets that will eventually leave the solar system, and one—Voyager 1, launched in 1997—that has already reached the solar system's edge.

Capelotti's (2010) catalog could become a starting point for problem-oriented research, and he ventures several possibilities. One slightly far-out suggestion is to envision alternative approaches to space exploration that might be created by an alien intelligence (p. 9). A more conventional problem is to learn how humans develop technologies for exploring "the extreme environments in space" (p. 15). He also highlights the value of studying failed missions (see below).

[9]The rovers: http://en.wikipedia.org/wiki/Mars_Global_Surveyor, accessed 9 June 2012; Capelotti (2010:129); Viking I: http://www.msss.com/mars_images/moc/4_14_98_vl1_release/, accessed 9 June 2012, and Capelotti (2010:117).

In documenting the archaeological record *beyond* Earth, Capelotti properly excluded dozens of failed American and Soviet missions that resulted in debris deposited on Earth, such as a rocket's explosion on the launch pad or a crash after failing to reach escape velocity. Although no doubt dispersed in a variety of deposits and perhaps missing materials removed by scavenging, artifacts may be encountered during surveys of launch areas (see below) or in undersea exploration. Other missions have also created a material record that may be of interest. A handful of American and Soviet craft departed on circumlunar missions, and some returned to Earth as planned; the latter craft exist as museum specimens. Evidently, the material record of space exploration is large and growing, and parts of that record may be available to Earth-bound archaeologists.

Research Possibilities

Can archaeologists create knowledge about the conduct of Space-Age science beyond documenting actual artifacts and sites? Gorman (2009c) answers affirmatively. We can do so, she suggests, by employing "the same approaches used to investigate the far distant past: chronological and technological trajectories, deep time spans, the influences of climate and landscape, sources of raw material, cultural exchange and cultural contact" (p. 133).

One general strategy is to explain patterns of technological change and variability. Much has been written about the design and construction of space suits, for example, and many specimens can be examined in museums, especially the National Air and Space Museum; oral history is also feasible. From these sources we could identify changes in materials and fabrication techniques that have transpired during more than half a century of development. We might explain these design changes in relation to experience gained from tests on Earth, their performance in space, the availability of new materials, symbolic functions, and the changing performance requirements of different missions. Three countries—the United States, Russia, and China—now make their own space suits. What factors explain the similarities and differences in each country's current design approach? And to what extent have they drawn on each other's generalizations and recipes? We need not stop with space suits, for any technology is fair game for comparative analysis and explanation, from gantry and launch pad design to the creature comforts afforded the first Earth-orbiting astronauts.

Gorman's (2005a, 2009a) studies of the Woomera Rocket Range's effects on indigenous Australians set an important precedent. She also wrote about the interactions between the French aerospace colonials and the indigenous Tuareg people on the rocket range in Colomb-Béchar, Algeria (Gorman 2009c). Perhaps employing ethnoarchaeology, we could perform similar studies in the USA where space facilities have encroached on traditional territories. Going further, Gorman (2009a:165) suggests that we may investigate "how space technology has contributed to the growth of global capitalist economies; and the participation in this economy of

people usually considered marginal to its operation." Indeed, she emphasizes the need to contextualize the technologies of space exploration in relation to other post-World War II socioeconomic trends such as decolonization and globalization (2009a).

NASA, other government agencies, and contractors, along with employees and admirers, claim that space technologies have spun off many civilian products. Such assertions are examples of "crypto-history," fact-like statements about technological history that the reader cannot readily evaluate (Schiffer 2011:16–17). Referring to Transit, the US Navy-sponsored precursor to the Global Positioning System satellite network, Darrin and O'Leary (2009a:7) assert that it "spurred the development of a rechargeable cardiac pacemaker, programmable implantable medication system, and automatic implantable defibrillators." To judge the validity of such a claim, we may ask:

1. What were the performance characteristics of the space technology?
2. Was this technology easily redesigned and commercialized to meet the performance requirements of civilian uses?
3. Did the original development project create components, generalizations, or recipes that became resources for the civilian technology?
4. What resources were actually required to develop the civilian product and what were their origins?
5. Might the civilian technology have been independently developed and commercialized?
6. If the space technology made only a marginal contribution to development of the civilian technology, can we explain how the spinoff myth arose and was perpetuated?

Although space exploration has obviously generated innumerable aerospace technologies, specific claims for *civilian* spinoffs require rigorous evaluation, any one of which could become an interesting research project.

In creating demands for specialized radio and video transmitters, optical components, photocells, and so forth, did space exploration stimulate the development of new industries? If so, what were they? To what extent was an industry developed by established aerospace companies versus start-ups or firms that made unrelated technologies? Which industries and companies prospered? Did such firms eventually use their technologies to commercialize consumer products?

Employing specially designed instruments, space exploration has made contributions to virtually every physical and biological science. Focusing on a particular environmental variable, such as the shape of the Earth's magnetosphere or the gravitational field of other planets, archaeologists could describe and explain change and variability in the instruments that measure it. We could also examine how increasing knowledge of an environmental variable, such as the scorching temperature of Venus' surface, redounded on spacecraft design (Alice Gorman, personal communication, 2012).

Many missions to the Moon and Mars ended in failure, with the lander either crashing or falling silent, in both cases creating one or more sites. If retrieval of remains from such sites becomes feasible *as well as legal and ethical*, their forensic

study could help to identify the failure's cause. With the secretive Soviet lunar probes in mind, Capelotti (2004) suggests that the spacecraft's actual components could be compared with those previously made public. Beyond satisfying historical curiosity, solving the mystery of a mission's demise might, some say, assist in designing future missions (Spennemann and Murphy 2009). In any event, the archaeological contribution would be to design a state-of-the-art recovery process. Engineers and other specialists would undertake the failure analysis, but a well-documented archaeological record is a useful—even essential—line of evidence (Spennemann and Murphy 2009).

The provenance of the parts that went into any US spacecraft, satellite, or probe could also be investigated. A comparative study of the innards of a well-dated class of hardware that included items returned to Earth as well as those that never left might indicate trends and variability in procurement activities. One hypothesis is that US manufacturers of space hardware, like the manufacturers of consumer goods (Schiffer 1991), used an increasing number of parts made in foreign countries.

China, India, and Japan have established their own space programs, launching satellites and preparing for more ambitious missions. Their satellites, as well as those launched decades earlier by the USA and Soviet Union, are gathering data on a variety of geophysical and atmospheric phenomena. Are these new satellites creating new knowledge or merely making redundant observations? Do these missions serve mainly to advertise technological prowess? To what extent do these countries rely on the scientific and technological resources created by the USA and Soviet Union?

The plaques and similar official objects that accompanied probes expected to leave the solar system, such as Pioneer 10 and 11 and Voyager 1 and 2 (Clemens 2009), would make an interesting data set. Designed for viewing by an alien intelligence, these objects could be analyzed symbolically. What cultural assumptions underlay the choice of placement, objects, materials, and images? How did mission planners arrive at decisions about these tokens of humanity? Did manufacturers of the satellites and probes insert any unauthorized objects?

US astronauts sometimes brought along items on orbital and lunar missions. Using oral history and other sources, a researcher could compile an inventory of these items carried aloft on Mercury, Gemini, Apollo, and the Space Shuttle. Did the kinds of objects exhibit any consistency within these mission groups? Over time, from group to group, did the kinds of objects change? Explanations can be offered for any discernible patterns. In addition, we can perhaps learn about the objects that astronauts from other countries took to space. Are there international differences?

Final Thoughts

The archaeology of the Space Age is a recent development that seemingly has an inaccessible subject matter. To the contrary, this chapter has shown that archaeologists have been making creative contributions employing diverse sources of

evidence, from documenting launch pads and other facilities on military bases to interpreting satellite images of lunar sites. In addition, archaeologists have raised fascinating preservation issues about satellites in orbit and Apollo and other landing sites on the Moon. As the V-2, Apollo, and other programs fade in popular memory and culture, archaeologists will continue to furnish new insights into them by querying the copious surviving artifacts and documentary evidence. Perhaps space is the final *archaeological* frontier (Capelotti 2004).

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Discovery Processes: Trial Models

I suggest that crafting generalizations about the processes of discovery, communication, and evaluation should have a high priority in the archaeology of science. Our aim would be to contribute, generalization by generalization, to a behavioral science of "sciencing" (to use White's [1949:3] neologism). Sociologists, philosophers, and others have established the foundations of this science, but an archaeological approach would, by privileging people–artifact interactions in activities (e.g., Edgeworth 2012), craft generalizations about processes that crosscut different projects, investigators, discoveries, time periods, social groups, and polities. In this chapter I identify—and generalize in behavioral terms—several apparatus-intensive discovery processes, some of which are already known to students of science. (See Klahr et al. [2000] for a decision-making perspective on discovery.)

Although biographies, histories of discoveries, and histories of disciplines discuss *specific* discoveries, there are few treatments of discovery as *a set of patterned empirical processes susceptible to generalization*. One reason for the dearth of generalizations is that many researchers believe a discovery results from the investigator's personal history, psychological processes (e.g., Hanson 1958), or an ill-defined "social milieu," and thus are amenable only to biographical, psychological, or cultural analysis. Another reason is that reports of experiments chronicle an idealized sequence of events unrelated to the messiness, backtracking, and dead ends encountered in actual projects (Beveridge 1958:111; cf. Hall 1956:168). Gooding (1989:64) notes that published experiments "reflect the plan or the finished product, rather than actual practice," and so they often leave out details of crucial interactions. Fortunately, laboratory notebooks, anecdotes, autobiographical accounts, correspondence, oral history, and comparative studies make it possible to research the materiality of *some* discoveries in detail; and, significantly, we may also repeat experiments (e.g., Cavicchi 2006; Gooding 1990a). But it remains for us to systematize—and generalize about—common discovery processes that may be abstracted from the details of specific projects.

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A behavioral approach to discovery requires, ideally, that we explain a discovery by invoking relevant contingent factors (of the investigator and of the societal context) while placing it in a class of discoveries exhibiting a similar pattern of people–artifact interactions—as described by a model. Discovery processes ought to exhibit many patterns whose descriptions require many models. As an outcome of archaeological research, discovery models are generalizations about a class of investigator–apparatus interactions that yield new scientific knowledge.

What Is a Discovery?

Discovery is both a material process and a social process. As a material process—the subject of this chapter—discovery begins when an investigator observes, in a new activity, a previously unknown phenomenon or effect. (A "new" activity has a unique constellation of apparatus and interactions, perhaps derived from an existing activity through deletions, additions, or substitutions.) If the investigator attributes significance to the observation and applies to it a new description or generalization (see chapter "Science: A Behavioral Perspective"), then the result is a discovery claim or provisional discovery. A discovery claim refers to an empirical phenomenon, whether or not it is mediated through, or created by, apparatus.

As a social process, discovery begins when the investigator communicates the claim to other people in a community of practice (Barnes, Bloor, and Henry 1996; Golinski 1998). If the claim survives evaluation according to the group's standards, which include prevailing theories, the new description or generalization is accepted as a discovery. Communication of a discovery claim is essential: an observation kept secret by one tribal member but not passed on is no different from an observation moldering forgotten in a laboratory notebook: neither is a discovery (cf. Gooding 1990b:154). Thus, some of Henry Cavendish's findings on electrical phenomena, recorded in notebooks in the late eighteenth century but not shared with others, had to be discovered again and published by investigators in later decades (Maxwell 1967). Some researchers carry the communication criterion further, insisting that a discovery claim must be made *public*, as in a journal article, allowing scrutiny by any investigator. According to this view, there can be no proprietary or secret science.

The make-it-public criterion is problematic from an archaeological standpoint. In inferring the knowledge embodied in making or using a prehistoric technology, we may be modeling the science of a specialist producer, as in a community's only metal worker. In such cases the knowledge may be implicit, incapable of being verbalized, yet is communicated to others as discovery-informed recipes, perhaps in an apprenticeship context. Passing a recipe from generation to generation meets the communication requirement. Also meeting this requirement is proprietary science, the trade secrets of a company disseminated to new employees. Likewise, the secret science of weapons research is passed down within government laboratories and contractors. It would be a mistake to rule out discoveries that are communicated

exclusively within a specific organization (e.g., family, lineage, guild, community, government laboratory, corporation) merely because they cannot be learned by outsiders. Such a move would unduly restrict the scope of science studies, the archaeology of discovery in particular. The make-it-public criterion appears to be relevant mainly in the context of modern, publicly funded civilian science. Thus, in circumscribing the domain "scientific discovery," I retain the communication criterion but reject the make-it-public requirement.

Although I strongly advocate the construction of discovery models, I respect the contingencies that make each discovery unique and interesting. Indeed, I emphasize that contingent factors and the pertinent model are both necessary to explain a discovery claim. Let us now turn to a sample of discovery processes and models, the latter in various stages of refinement.

Accident, Serendipity, and Chance

"Many, if not all, scientific discoveries are made by a kind of inspiration fastening on an accident" (Pye 1978:62; see also Beveridge 1958, chapter 3). Perhaps an exaggeration, Pye's assertion nonetheless underscores the role that accidents and chance play in the genesis of much scientific knowledge. Taton (1957) thought deeply about the role of chance, which he defined as an "exceptional concurrence of circumstances" (p. 79). This idea can be stated in behavioral terms: an accidental discovery begins when a configuration of apparatus and interactions—perhaps unplanned—yields an unexpected but noteworthy effect; it need not be "exceptional" (see also Blackwell 1969:65–67). "Noteworthy," I emphasize, depends on the investigator taking notice: "accidents became discoveries because of the sagacity" of the investigator (Roberts 1989:244).

A celebrated case of accidental discovery led to a generalization about the storage of electrical charge.[1] The discovery took place in the laboratory of eighteenth-century natural philosopher and instrument maker Petrus van Musschenbroek of Leiden University. A frequent visitor to Musschenbroek's laboratory was Andreas Cunaeus, a lawyer captivated by electrical experiments. One day in 1746, seeking to repeat at home a common experiment—electrifying water—Cunaeus held a jar of the liquid in his hand and placed it in contact with the prime conductor of an electrical machine (electrostatic generator). Testing the charge on the water with his other hand—a novel interaction—Cunaeus found that it was horrifically greater than he expected.

Informed by Cunaeus about this surprising performance, Musschenbroek tried the experiment himself, using a glass globe in place of the jar. The result was the same: the professor received a hefty jolt. Musschenbroek quickly published his findings, and electrical experimenters throughout the West repeated and embellished the experiment. Among the replicators was Benjamin Franklin, who showed that the charge actually resided on the glass. Thus, a simple charge-storage

[1] The Leyden jar case study is adapted from Schiffer, Hollenback, and Bell (2003:44–47).

device—today called a condenser or capacitor—could be made by placing an insulating material between two conductors. Connecting the conductors to a source of electricity "charges" the capacitor. When the insulator was a glass or ceramic vessel, it came to be called a Leyden jar. Computers and smart phones contain billions of microscopic capacitors, employing the fundamental effect that arose by accident.

This accident, I maintain, was waiting to happen because many experimenters imparted charge to water in a glass vessel. Given that hundreds of people were playing with electricity in the eighteenth century, it is highly probable that others would have stumbled upon the inadvertent interactions that produced the unexpected effect, and a few would have believed the effect to be significant. Indeed, a German experimenter reported the effect at the about the same time as Musschenbroek, but his muddled description was ignored.

Every accidental discovery occurred with a certain probability that might be estimated today. We would need to consider necessary conditions, such as the kinds of places where components of the apparatus resided together, how many such places there were, and the complexity of the interactions needed for the performance. Also relevant is the prevalence of people who, by virtue of training and experience (i.e., possessing the proverbial "prepared mind"), could attribute significance to the performance and generalize about the conditions that produced it. By merely taking these factors into account without doing actual calculations, we could assess, for example, which effects were more or less likely to occur accidentally than others.

It would be instructive to identify effects that arose by accident even though they were highly improbable. By the same token, when the factors conducive to creating a particular performance abound, we may expect, as in the case of the Leyden jar, independent discoveries. We could take the latter statement as a hypothesis, and investigate in a large sample of accidental discoveries whether the number of discovery claims is correlated with effects generated by highly probable accidents. Does the degree of acclaim achieved for a discovery depend on whether the effect was produced by a probable or improbable accident?

Trial and Error

According to Campbell (1960), Alexander Bain used the phrase "trial and error" as early as 1855. Since then it has come to describe a discovery process that aims at a specific outcome but lacks the apparatus and/or generalizations to achieve it directly. Thus, trials—alterations of apparatus and interactions—take place, which may be based on hunches, intuition, or other inexplicit guides. The immediate outcome may be a failure, which then leads to more trials or project abandonment.

Trial and error often involves *backtracking*. Suppose that a modern craft potter wants to make, for the first time, a very large and complicated sculpture. Employing a paste composition used for making small vessels, she discovers that the sculpture warps and cracks while drying in the open air. To remedy this problem, she covers her

next sculpture with damp cloth and allows it to dry more slowly. The results are better, but the sculpture still deforms noticeably and exhibits some cracks. On her next trial, she covers the sculpture tightly with layers of plastic, unwrapping it for an hour or so every day. Again she sees a slight improvement yet the result is still unsatisfactory. At last she backtracks by choosing a different paste composition, one containing an unusually large amount of sand. This time the slowly dried sculpture is free of flaws. In backtracking, the investigator returns to, and modifies, earlier technical choices.

The historical record of technology development furnishes many examples of backtracking. Edison, for one, devoted immense effort to making a platinum filament for his incandescent lamp, building into countless bulbs many electromagnetic devices supposed to keep the filament from melting. None worked well. Finally he backtracked by turning to organic materials, which eventually succeeded (Friedel, Israel, and Finn 1986).

A variant of trial and error is the shotgun or "brute force" process. Edison's search for a workable filament material after he abandoned platinum is the archetypical example. Trials with thousands of materials all failed to meet minimum performance requirements until he tried bamboo, which showed great promise. A worldwide search then ensued for the best bamboo variety, which turned out to be "Japanese" bamboo (Freidel, Israel, and Finn 1986:157). This search concluded with a useful generalization: a thin strip of Japanese bamboo, when bent into a horseshoe shape and heated in a muffle furnace, becomes a filament of high resistance, capable of reaching incandescence, that neither burns up nor fails quickly when current is passed through it. Edison also used the shotgun approach to find the best electrode materials for his alkaline storage battery, which led to a brilliant success (see chapter "Thomas Edison's Science").

Instead of concluding that trial and error is an inferior discovery process, wasteful of resources, we should examine each project closely to learn if relevant generalizations had been established previously but were not employed. Only in the latter case may we conclude that trial and error was inappropriate. Although Edison has been criticized for being an atheoretical empiricist in using brute force to solve problems, in neither the filament nor the battery projects had relevant generalizations been available beforehand (Carlson 1988; Friedel, Israel, and Finn 1986; Vanderbilt 1971).

Trial and error is no doubt more common than published reports indicate, for admitting its use may suggest that a project was built on ignorance and wasted resources. Yet, in the absence of relevant generalizations, the process has been effective. I suggest that trial and error was the predominant discovery process in traditional societies and remains a major one in industrial societies.

Trial and Assess

A more cognitively complex version of trial and error is trial and assess. Each trial is followed by an assessment that extracts useful information from intermediate outcomes and guides the next trial. In effect, trial and assess does not result in errors

but in what Gooding (1990b:159) calls "informative failures" (see also Cavicchi 2006). It is clearly a more purposive process than trial and error, does not operate in total ignorance, and may involve continuous hypothesis testing. Trial and assess perhaps prevails when the investigator seeks a path to a well-defined outcome having a specifiable endpoint (see also Klahr et al. 2000, chapter 2).

As an example of this process, let us examine an especially consequential electrical project of the nineteenth century: Michael Faraday's discovery that magnetism can create electricity (Faraday 1952). Many investigators sought this outcome during the 1820s after Oersted showed that current coursing through a wire produces magnetism. This process, many experimenters reasoned, should be easy to reverse, but until Faraday's project all previous efforts had failed.[2]

Oersted's discovery was also the starting point for the construction of electromagnets—essentially a coil of insulated wire wound upon a core of soft iron. Faraday began his project by making and playing with electromagnets, which he thought might bring about the long-sought effect. On August 19, 1831, in an experiment hinting that success might be near, Faraday wound separate coils on an iron ring. First he connected one coil to a meter. Then, upon connecting the other coil to the battery, he noticed something surprising: the meter's needle deflected at the precise instant when he made or broke contact with the battery. After oscillating briefly, the needle became quiescent. Others had observed this effect but attached no significance to it because it did not produce a *steady* current. However, Faraday believed that this effect provided important information and he explored it further.

In one set-up with two coils, he found that the needle also deflected when the second coil was moved toward or away from the first. This presumably led Faraday to hypothesize that relative motion between a magnet and a coil might play a role in producing electricity. Thus, he tried thrusting a permanent bar magnet into a hollow coil and withdrawing it quickly. As he suspected, both motions caused the meter's needle to move. Further experiments of this kind yielded identical results, and so he concluded that the needle moved only during a brisk motion of the magnet relative to the coil. Because Faraday also sought a steady current, he did not yet report these findings.

The next stage of his project incorporated an apparatus used by François Arago, a Frenchman, who had found in the mid 1820s that a rotating metal disk moved the needle of a nearby compass—i.e., it produced magnetism. Faraday apparently intuited from Arago's experiment and from his own earlier findings that electricity might be induced continuously in a metal disk rotating near a magnet. On October 28, 1831, after dozens of trials that failed to yield the effect, Faraday at last found a configuration that worked. He began with a copper disk, 12 in. in diameter and about 0.2 in. thick, mounted on an axle held in a frame. Next he placed the copper disk vertically between two small iron bars attached to the poles of an enormous permanent magnet. Finally, he connected one wire of the meter to the axle and held the other wire against the rim of the copper disk as it turned. What happened next is best described in Faraday's own words: "the instant the plate [disk] moved, the galvanometer was

[2]The Faraday case study is adapted from Schiffer (2008:50–51).

influenced, and by revolving the plate quickly the needle could be deflected 90°. Here therefore was demonstrated the production of a permanent current of electricity by ordinary magnets" (Faraday, in Martin 1932:279). After further experiments, Faraday offered additional generalizations about the process of converting magnetism into electricity. Philosophical instrument makers promptly exploited Faraday's generalizations and offered for sale the first electromagnetic generators.

At the beginning of the project Faraday had envisioned a specific outcome: producing electricity continuously by means of magnetism. This goal gave direction to the trials and inflected his assessments of intermediate outcomes. Of Faraday's capacity to perceive the import of intermediate trials while holding resolutely to an objective, John Tyndall (1868:20) wrote that "The intentness of his vision in any direction did not apparently diminish his power of perception in other directions; and when he attacked a subject, expecting results, he had the faculty of keeping his mind alert, so that results different from those which he expected should not escape him through pre-occupation." Faraday also studied the findings of other investigators and often repeated their experiments. Arago's experiment, for example, inspired Faraday to reconfigure his own apparatus and provoked hunches and hypotheses that led to the coveted outcome. Because Faraday reported many trials, regardless of outcome (and his notebooks have also been published), his experiments can be reconstructed in unusual detail.

Faraday's path to "magneto-electricity" was neither direct nor rapidly traversed, and it was a discovery process more cognitively complex than trial and error. The paths to many other discoveries, some of them equally momentous, conform broadly to the trial and assess process.

Readers may wonder why Edison's filament-discovery process is not an example of trial and assess. After all, it did have a well-defined objective: find a material that will make a long-lasting filament that reaches incandescence. However, it lacked a specifiable endpoint. It was simply a matter of trying one material after another until Edison was satisfied with the material that, among all trials, performed best; it might have been the first one tested or the last. The project was open ended: Edison could have continued testing more materials, perhaps finding one superior to "Japanese" bamboo. Moreover, we may suppose that the only assessment that took place after each trial was that of comparing the performance of the new material with those of previous trials. There was simply no prior, independent basis for deciding that the project was done. Faraday's project, on the other hand, began with a specific objective along with a well-defined endpoint. Once he had had produced a continuous electric current from magnetism, which depended on informed assessments after each trial, Faraday proclaimed his discovery, communicated it to others, and it rapidly became a cornerstone of electrical science and technology.

Discovery Machines

A *discovery machine* is an apparatus whose components are capable of undergoing many substitutions, one at a time, each of which may generate a new effect (compare to Shapin's [1996:96] "fact-making machine" and Golinski's [1992:9] "engine

of discovery"). Let us make this definition more concrete by envisioning an apparatus of near ultimate simplicity: two components interacting only with the investigator and each other. We presume that during the apparatus' first uses there arises an effect that becomes a discovery. To transform the apparatus into a *discovery machine*, the investigator makes a series of substitutions for one of the components or undertakes a series of new interactions. In this manner, the ever-changing apparatus, *still possessing the same basic structure*, yields one new effect after another, some of which may also become discoveries. A discovery machine, then, generates a *discovery cascade*.

A straightforward implication of the discovery machine model is that, after the structure of a new discovery machine is reported, other investigators will acquire one and conduct new experiments. A further implication is that some of those investigators may make the same, somewhat obvious substitutions, and thus arrive independently at the identical discovery. Thus, Volta's electric pistol, a reaction chamber that combusted gases, rapidly became a discovery machine, several of whose users independently learned that burning hydrogen in oxygen created water (Schiffer, Hollenback, and Bell 2003:217–222).

The key to understanding the development of a discovery machine is in appreciating the importance that investigators assign to an apparatus' early successes. These discoveries are taken as an auspicious sign that other significant effects are in the offing, merely awaiting the right substitutions. As the examples below demonstrate, a successful discovery machine raises the probability of future discoveries in a satisfying and sometimes dramatic fashion.

One of the most prolific discovery machines has been the optical microscope. Robert Hooke, a British instrument maker and polymath, designed a compound microscope, and in 1665 published the highly influential *Micrographia* that reported his observations (Jardine 2004). Hooke's discovery machine consisted of the microscope, the viewed object and tools to manipulate it, and his interactions with them. Hooke's strategy was to view a variety of objects and document them with remarkably detailed drawings (Fig. 1). In operating the discovery machine, he observed fish scales, a bee's stinger, insect eggs, a fly's eye, poppy seeds, a razor's edge, a needle's point, charcoal, and dozens more (Hooke 1665). Previously unknown, the fine details of each object, communicated in text and image, were discoveries eventually accepted far and wide. Since Hooke's time, new kinds of optical microscopes having enhanced performance characteristics, such as higher magnification, also became discovery machines. And beginning in the mid-twentieth century, there has been a parade of new microscope designs—and thus new discovery machines—from the scanning electron microscope to the scanning tunneling microscope, the latter capable of imaging single atoms.

In its earliest use for making astronomical observations by Galileo, the telescope began its long career as a discovery machine. It consisted of the telescope, objects in the sky that emitted or reflected light, and Galileo's interactions (Cohen 1985). By pointing the telescope successively in various directions, Galileo brought the visual performance of different objects into view; these observations he dutifully recorded and published. Although scarcely equal in resolving power to an

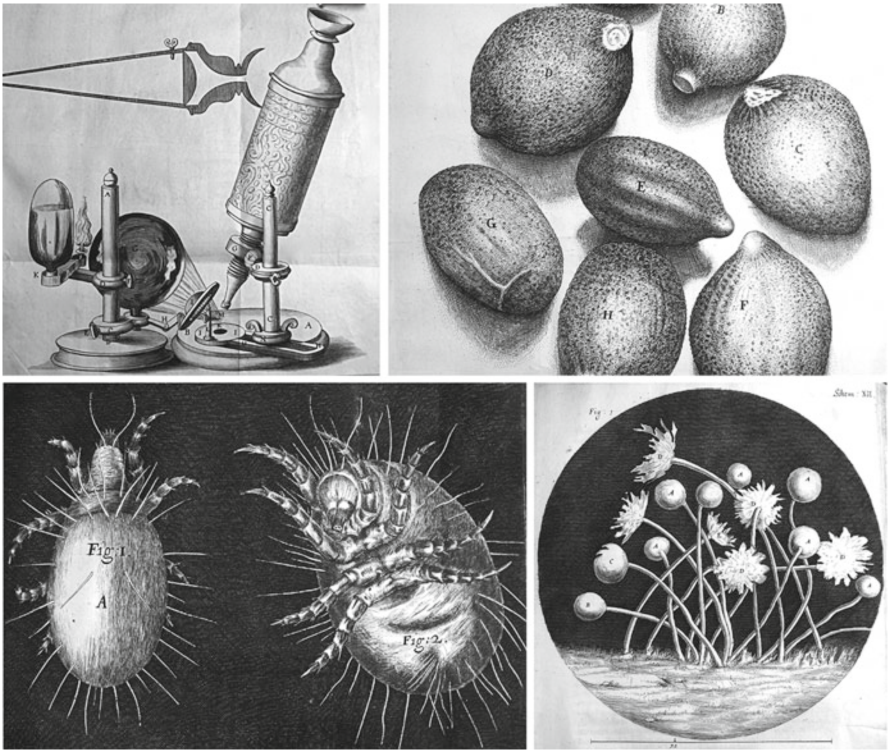

Fig. 1 (*Upper left*) Hooke's microscope; (*upper right*) thyme seeds; (*lower left*) mites; (*lower right*) growths on a leaf (adapted from Hooke 1665 in the Dibner Library, Smithsonian Institution)

inexpensive child's telescope of today, Galileo's telescope yielded significant observations that became discoveries, including moons of Jupiter, Saturn's rings (which he called "ears"), sunspots, and features on the Moon. As in the case of the microscope, improvements in the optical telescope's performance characteristics over the centuries enabled each new kind to become a discovery machine.

The electrolytic cell is also a discovery machine.[3] It consists of a glass or ceramic container, a conductive solution, two electrodes—one of which is the conductive object to be plated, and a source of low-voltage, high-current dc electricity such as a hefty battery. The foundations of this discovery machine were laid in the late eighteenth and early nineteenth centuries after it was learned that electricity could promote chemical decomposition. Specifically, Humphry Davy and others showed that a battery's current could liberate metals from their compounds. In 1831, trying to better understand electrical decomposition, the Italian Carlo Matteuci used a 30-cell battery to release copper, silver, lead, and other metals.

[3]The electrolytic cell discussion has been distilled from Schiffer (2008, chapter 8).

The diseconomy of refining a common metal from its ore by electrolysis was obvious, but a new application with greater commercial potential was invented independently toward the end of the 1830s by several investigators, including Thomas Spencer in London and Moritz Jacobi in Russia. They reconceptualized electrical decomposition as *electrometallurgy*, a process for both depositing and *shaping* metal. In a solution of copper sulfate, a layer of copper could be deposited on any object that served as one electrode, so long its surface was conductive. Both investigators published their findings, and the electrolytic cell rapidly became a discovery machine, as investigators in many countries substituted varied solutions that electrodeposited different metals. Within several decades dozens of recipes had been patented, some of them quite complex and requiring much chemical expertise. One recipe for plating gold used a heated solution of distilled water, gold chloride, sodium phosphate, sodium bisulfite, and potassium cyanide. Recipes for plating copper, silver, gold, and nickel, especially, became the foundation of new industries.

Discovery machines exhibit different patterns of development. For the microscope and telescope, the developmental distance to create the first apparatus was considerable, as special lenses and devices to hold them had to be made, but for each subsequent discovery the developmental distance was trivial. Not so the electrolytic cell, whose initial development for plating copper merely required the assembly of inexpensive, off-the-shelf materials. However, solutions for plating some metals necessitated much chemical expertise and many trials and assessments. I surmise that the greater the developmental distance needed to achieve the initial discovery, the more heroic the project is likely to seem, and this may earn for the investigator special rewards.

A vast number of discovery machines have appeared in early modern and modern science, any one of which might make an interesting research project focused on its development and use. Among those coming quickly to mind are Geissler tubes, X-ray apparatus, spectrometers, and gene sequencers. In earlier societies, technologies such as cooking pots, pipes for smoking, and distillation apparatus might be analyzed as discovery machines.

Technology Transfer

A technology developed in one societal context may be taken up by investigators in another context, perhaps redesigned to perform in new activities, and put to work in search of novel effects. Common in modern science, technology transfer has a long history. The first telescopes, after all, were not developed for astronomical observations but for naval use; and prisms were used by natural magicians before Newton obtained one and studied the properties of light.

As discussed in the chapter "The Apparatus of Modern and Early Modern Science," Francis Hauksbee crafted an electrical machine in the first decade of the eighteenth century. In the following decades, this technology was tried out in many

activities in diverse societal contexts, and easily crossed international borders. The machine furnished a source of (static) electricity for experiments in physiology and medicine, chemistry, earth sciences, and so forth (Schiffer, Hollenback, and Bell 2003). In each new context, communities of practice formed around the machines, which people sometimes had to modify; portable versions, for example, were developed for medical applications. After observing new effects, investigators published their findings, some of which were recognized as discoveries, including electricity's abilities to decompose and synthesize chemical compounds, accelerate plant growth, and force muscles to contract.

For present purposes, we may model technology transfer as a three-phase process.[4]

1. In *information transfer*, people learn about a technology through word of mouth, written materials, or examples of the apparatus itself. We may suppose that communication and transportation networks determine the size and spatial distribution of potential recipient communities.
2. *Experimentation* involves an assessment of the new technology's performance characteristics in relation to the recipient group's activities—current and anticipated. This phase begins when people try out the new technology in ongoing activities that differ from those of the originating community, or they may forecast in "thought experiments" how it might perform in a new activity. Early experiments may indicate that a technology shows promise but its weighting of performance characteristics is unsuitable.
3. *Redesign* creates new variants of the technology that more closely match the performance requirements of the recipient community's activities. The manufacture and operation of each variant requires new generalizations—recipes at the very least. And a new technology itself may become a discovery machine.

Technology transfer, which has accelerated since the early nineteenth century as a result of new communication and transportation modes, has been a fecund source of discoveries. Informative case studies of technology transfer could be built upon X-ray machines, spectrometers (of several varieties), lasers, and particle accelerators, all of which became discovery machines in new contexts.

Taking the Next Step

Sometimes a very productive apparatus, having generated many discoveries, no longer yields new effects. Investigators eager to further this line of research may conclude that additional effects are in the offing, and perhaps discoveries, if critical performance characteristics can be dramatically enhanced with new apparatus (cf. Greenstein 1998:179–180). Taking this next step usually requires a major alteration

[4]The three-phase model is an abridgment of a six-phase model (Schiffer, Hollenback, and Bell 2003:176–180).

of the apparatus, sometimes the creation of an entirely new one having a great developmental distance. The next step may be taken by the group that took the previous step or by another group, depending on available resources, incentives such as peer competition, and rewards for making further discoveries (cf. Holton 1998:431). It should be noted that each step may produce a new discovery machine.

Examples of the next-step model abound in early modern and modern science. Hauksbee's electrical machine created many new effects, but some investigators suspected that more powerful machines would be more productive. Accordingly, they built new kinds of machines, often very large ones, throughout the eighteenth century (Hackmann 1978). This trend culminated in van Marum's machine, completed in 1784 (see chapter "The Apparatus of Modern and Early Modern Science"). He believed that "if one could acquire a much greater electrical force than hitherto in use, it could lead to new discoveries" (van Marum, quoted in Heilbron 1979:441). Although it produced a spark about 2 ft long and was employed in many useful experiments, it yielded no new physical effects. As in this case, the next step may be the last step; even so, construction and use of the monster machine resulted in new recipes. Not until the twentieth century, however, did new electrostatic generators (e.g., Van de Graaff machines) produce new effects.

Since Galileo's time, each step in the development of more powerful optical telescopes has often required a major technology project (Andersen 2007). Newton and others took an early step by making a reflecting telescope, using mirrors instead of lenses to collect and focus light. When the size limits to forming a one-piece glass mirror using conventional processes had been reached in the twentieth century, investigators took steps in several directions. One was the development of spin-casting technology, which makes a large but lightweight mirror in a rotating furnace—a forming process that can last for months. Other projects created telescopes with multiple mirrors whose images are combined electronically; still others made segmented mirrors. The literature on telescope history and design is enormous and might serve as a resource for archaeological studies.

Another example is the construction of larger and more powerful particle accelerators for studying the ultimate composition of matter (Close, Marten, and Sutton 2002). As Sessler and Wilson (2007:xi) note, "The appetite of particle physicists for particles of higher and higher energy seems never to be satisfied." The first accelerator, Ernest O. Lawrence's cyclotron, was small enough to repose on a chair. Lawrence himself went on to build at Berkeley a succession of ever-larger and more powerful machines that yielded a stream of new effects that fed on and fed into theories of nuclear and particle physics. The trend toward gigantism in accelerators continued throughout the twentieth century in laboratories around the world, driven in part by peer competitions among investigators, institutions, and countries. The trend's most recent materialization is CERN's Large Hadron Collider, an international undertaking costing more than $10 billion (see chapter "Science: A Behavioral Perspective"). In mid-2012, CERN announced that it had obtained strong evidence that its major quarry, the Higgs boson, had been found.

Studies using the next-step model would pay close attention to discerning the social processes, such as peer competition, that both impelled the next step and

furnished the necessary resources. In making this move, we can show how new apparatus is the essential link between social processes and specific discoveries. Perhaps we could obtain insights into the historical trajectory of many scientific apparatus by coupling the next-step model to the saltation model of technological change (Schiffer 2011, chapter 11).

Discussion

The discovery models presented above are based on early modern and modern science, garnered from written materials. As such they are built on a somewhat narrow empirical base. Clearly, many discoveries took place in nonliterate societies. Even in the twenty-first century, some artisans in industrial societies, including potters, woodworkers, and glassblowers, invent recipes—and associated generalizations—and communicate them only verbally. Archaeologists, however, work with the entire material record, historical and archaeological, which encompasses the myriad discoveries of the past and present. In exploiting this vast material record, we may pursue three strategies: (1) evaluate whether the models above apply to discoveries in traditional societies, (2) seek patterns of discovery in traditional societies that require the creation of new models, (3) continue to create models that may be most applicable to discovery patterns in post-1600 science.

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